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Contents of No. 14.

	PAGE.
The Air of Stove-Heated Rooms. By W. Mattieu Williams	285
Young on the Sun. By the Editor.	286
Found Links.—Part III. By Dr. Andrew Wilson, F.R.S.E., &c.	288
Intelligence in Animals	289
Nights with a Three-inch Telescope. By "A Fellow of the Royal Astronomical Society." (*Illustrated.*)	290
Brain Troubles: Partial loss of Speech	291
Reviews: Science Ladders—Science for All—The Science of the Stars.	292
The Effects of Tobacco.—Part I. By Dr. Muir Howie	292
Professor Grant on Meteorology	294
Easy Lessons in Blowpipe Chemistry. —Lesson II. By Lieut.-Colonel W. A. Ross, late R.N.	295
Effluvia and Health; The Use of Fleas, &c.; Fish "Sounds." By W. Mattieu Williams	295
Correspondence: Telescope —Volcanic Projectiles, &c.	296-302
Star Map for February	297-300
Queries	303
Replies to Queries	303
Answers to Correspondents	304
Notes on Art and Science	306
Our Mathematical Column	307
Our Chess Column	308
Our Whist Column	309

THE AIR OF STOVE-HEATED ROOMS.

By W. Mattieu Williams.

WHATEVER opinions may be formed of the merits of the exhibits at South Kensington, one result is unquestionable—the exhibition itself has done much in directing public attention to the very important subject of economising fuel and the diminution of smoke. We sorely need some lessons. Our national progress in this direction has been simply contemptible, so far as domestic fireplaces are concerned.

To prove this we need only turn back to the essays of Benjamin Thompson, Count of Rumford, published in London just eighty years ago, and find therein nearly all that the Smoke Abatement Exhibition *ought* to teach us, both in theory and practice—lessons which all our progress since 1802, plus the best exhibits at South Kensington, we have yet to learn.

This small progress in domestic heating is the more remarkable when contrasted with the great strides we have made in the construction and working of engineering and metallurgical furnaces, the most important of which is displayed in the Siemens regenerative furnace. A climax to this contrast is afforded by a speech made by Dr. Siemens himself, in which he defends our domestic barbarisms with all the conservative inconvincibility of a born and bred Englishman, in spite of his German nationality.

The speech to which I refer is reported in the "Journal of the Society of Arts," Dec. 9, 1881, and contains some curious fallacies, probably due to its extemporaneous character; but as they have been quoted and adopted not only in political and literary journals, but also by a magazine of such high scientific standing as *Nature* (see editorial article Jan. 5, p. 219), they are likely to mislead many.

Having already, in my "History of Modern Invention, &c.," and in other places, expressed my great respect for Dr. Siemens and his benefactions to British industry, the spirit in which the following plain-spoken criticism is made will not, I hope, be misunderstood either by the readers of Knowledge or by Dr. Siemens himself.

I may further add that I am animated by a deadly hatred of our barbarous practice of wasting precious coal by burning it in iron fire-baskets half buried in holes within brick walls, and under shafts that carry 80 or 90 per cent. of its heat to the clouds; that pollute the atmosphere of our towns, and make all their architecture hideous; that render scientific and efficient ventilation of our houses impossible; that promote rheumatism, neuralgia, chilblains, pulmonary diseases, bronchitis, and all the other "ills that flesh is heir to" when roasted on one side and cold-blasted on the other; that I am so rabid on this subject, that if Dr. Siemens, Sir F. Bramwell, and all others who defend this English abomination, were giant windmills in full rotation, I would emulate the valour of my chivalric predecessor, whatever might be the personal consequences.

Dr. Siemens stated that the open fireplace "communicates absolutely no heat to the air of the room, because air, being a perfectly transparent medium, the rays of heat pass clean through it."

Here is an initial mistake. It is true that air which has been artificially deprived of *all* its aqueous vapour is thus completely permeable by heat rays, but such is far from being the case with the water it contains. This absorbs a notable amount even of bright solar rays, and a far greater proportion of the heat rays from a comparatively obscure source, such as the red-hot coals and flame of a common fire. Tyndall has proved that 8 to 10 per cent. of all the heat radiating from such a source as a common fire is absorbed in passing through only 5 ft. of air in its ordinary condition, the variation depending upon its degree of saturation with aqueous vapour.

Starting with the erroneous assumption that the rays of heat pass "clean through" the air of the room, Dr. Siemens went on to say that the open fireplace "gives heat only by heating the walls, ceiling, and furniture; and here is the great advantage of the open fire;" and, further, that "if the air in the room were hotter than the walls, condensation would take place on them, and mildew and fermentation of various kinds would be engendered; whereas, if the air were cooler than the walls, the latter must be absolutely dry."

Upon these assumptions, Dr. Siemens condemns steam pipes and stoves, hot-air pipes, and all other methods of directly heating the *air* of apartments, and thereby making it warmer than were the walls, the ceiling, and furniture when the process of warming commenced. It is quite true that stoves, stove pipes, hot-air pipes, steam pipes, &c., do this: they raise the temperature of the air directly by *convection; i.e.*, by warming the film of air in contact with their surfaces, which film, thus heated and expanded, rises towards the ceiling, and, on its way, warms the air around it, and then is followed by other similarly-heated ascending films. When we make a hole in the wall, and burn our coals within such cavity, this convection proceeds up the chimney in company with the smoke.

But is Dr. Siemens right in saying that the air of a room, raised by convection above its original temperature, and above that of the walls, deposits any of its moisture on these walls? I have no hesitation in saying very positively that he is clearly and demonstrably wrong; that no such condensation can possibly take place under the circumstances.

Suppose, for illustration sake, that we started with a room of which the air and walls were at the freezing-point, 32° F., before artificial heating (any other temperature will do), and, to give Dr. Siemens every advantage, we

will further suppose that the air was fully saturated with aqueous vapour, *i.e.*, just in the condition at which some of its water might be condensed. Such condensation, however, can only take place by cooling the air below 32°, and unless the walls or ceiling or furniture are capable of doing this they cannot receive any moisture due to such condensation, or, in other words, they must fall below 32° in order to obtain it by cooling the film in contact with them. Of course Dr. Siemens will not assert that the stoves or steam-pipes (enclosing the steam, of course), or the hot air or hot water pipes will *lower* the absolute temperature of the walls by heating the air in the room.

But if the air is heated more rapidly than are the walls, &c., the *relative* temperature of these will be lower. Will condensation of moisture *then* follow, as Dr. Siemens affirms? Let us suppose that the air of the room is raised from 30° to 50° *by convection purely*; reference to tables based on the researches of Regnault, shows that at 32° the quantity of vapour required to saturate the air is sufficient to support a column of 0·182 inches of mercury, while at 50° it amounts to 0·361, or nearly double. Thus the air, instead of being in a condition of giving away its moisture to the walls, has become thirsty, or in a condition to *take moisture away from them* if they are at all damp. This is the case whether the walls remain at 32° or are raised to any higher temperature short of that of the air.

Thus, the action of close stoves and of hot surfaces or pipes of any kind is exactly the opposite of that attributed to them by Dr. Siemens. They dry the air, they dry the walls, they dry the ceiling, they dry the furniture and everything else in the house.

In *our* climate, especially in the infamous jerry-built houses of suburban London, this is a great advantage. Dr. Siemens states his American experience, and denounces such heating by convection because the close stoves *there* made him uncomfortable. This was due to the fact that the winter atmosphere of the United States is very dry, even when at zero. But air, when raised from 0° to 60°, acquires about twelve times its original capacity for water. The air thus simply heated is desiccated, and it desiccates everything in contact with it, especially the human body. The lank and shrivelled aspect of the typical Yankee is, I believe, due to this. He is a desiccated Englishman, and we should all grow like him if our climate were as dry as his.* The great fires that devastate the cities of the United States appear to me to be due to this general desiccation of all building materials, rendering them readily inflammable and difficult of extinction.

When an undesiccated Englishman, or a German endowed with a wholesome John Bull rotundity, is exposed to this superdried air, he is subjected to an amount of bodily evaporation that must be perceptible and unpleasant. The disagreeable sensations experienced by Dr. Siemens in the stove-heated railway cars, &c., were probably due to this.

An English house, enveloped in a foggy atmosphere, and encased in damp surroundings, especially requires stove-heating, and the most inveterate worshippers of our national domestic fetish, the open grate, invariably prefer a stove or hot-pipe-heated room, when they are unconscious of the source of heat, and their prejudice hoodwinked. I have observed this continually, and have often been amused at the inconsistency thus displayed. For example, one evening I had a warm contest with a lady, who repeated the usual praises of the cheerful blaze, &c., &c. On calling afterwards, on a bitter snowy morning, I found her and her daughters sitting at work in the billiard-room, and asked them why. "Because it was so warm and comfortable." This room was heated by an 8-inch steam-pipe, running around and under the table, to prevent the undue cooling of the india-rubber cushions, and thus the room was warmed from the middle, and equally and moderately throughout. The large reception-room, with blazing fire, was scorching on one side, and freezing on the other, at that time in the morning.

The permeability of ill-constructed iron stoves to poisonous carbonic oxide, which riddles through red-hot iron, is a real evil, but easily obviated by proper lining. The frizzling of particles of organic matter, of which we hear so much, is—if it really does occur—highly advantageous, seeing that it must destroy organic poison-germs. Under some conditions, the warm air of a room *does* deposit moisture on its cooler walls. This happens in churches, concert-rooms, &c., when they are but occasionally used in winter time, and mainly warmed by animal heat, by congregational emanations of breath-vapour, and perspiration —*i.e.*, with warm air supersaturated with vapour. Also, when we have a sudden change from dry, frosty weather, to warm and humid. Then our walls may be streaming with condensed water. Such cases were probably in the mind of Dr. Siemens when he spoke; but they are quite different from stove-heating, which increases the vapour capacity of the heated air, without supplying the demand it creates.

* In each of my three visits to America I lost about thirty pounds in weight, which I recovered within a few months of my return to the "home country" (of English-speaking nations).—Ed.

YOUNG ON THE SUN.

By the Editor.

In the beginning of my former notice, I remarked that, although Professor Young is one of those to whom science is very largely indebted for our present knowledge respecting the sun, few would suppose so from the treatise before us. It so chanced that, on the very day when that notice was passing through the press, a review of Prof. Young's book appeared in *Nature*, of which the opening paragraph ran as follows:—

"Since the method of artificial eclipses was introduced in 1868, Prof. Young, the author of the book under notice, has from time to time done good work in utilising the capital climate of his native country, and his relatively superior optical means, to confirm in many essential points, and to add a little shading here and there, to the bold outlines of the new science, for which we are indebted to his predecessors."

I must confess I read these lines with a sense of regret —almost of shame. It is not fitting that any student of science in this country should be ignorant of the services which our American fellow-workers have rendered in solar research, as in other departments of science. It would be something worse if, knowing what those services have been, they should seek to ignore them. But, apart from this, the reasoning involved in the slur cast on Professor Young's original work is ridiculous on the face of it. One might as reasonably say that the Herschels, utilising their larger telescopes, did good work by adding a little shading here and there to the bold outline of telescopic astronomy, for which we are indebted to their predecessors; or that Huggins, Secchi, Lockyer, and others, employing better spectroscopes, have usefully applied the general principles for which we are indebted to Fraunhofer. No one who considers what Young has done can fail to see that while even that part of his work which depended on the method of artificial eclipses is full of original

value (three of the most important observations by this method were made by him), he has done most valuable work outside this particular line of research. He was one of those who first demonstrated the gaseity of the solar corona; he first demonstrated the existence of the atmosphere of multitudinous gases existing close to the visible solar surface; we owe to him the recognition of nine-tenths of the lines of the solar sierra (*all* of those lines which were at all difficult to discover); and he invented the method (successfully applied by Respighi and Lockyer in 1871) of observing the corona with a slitless spectroscope; these, and a number of other researches, more than justify what I said last fortnight. It appears to me—I speak under correction—that a rival worker in the same field, like the editor of *Nature*, one who formerly held views opposed to those which Professor Young's original researches have established, should not have allowed the above-quoted paragraph to appear in the journal which, we must assume, he controls. Some critics might attribute the review to his pen, and point out that while science must always gain, both in progress and in tone, from emulation amongst scientific fellow-workers, the same cannot be said of the quality which is associated with "malice and all uncharitableness," and in reference to which the old proverb says that *Qui invidet minor est.* We must not suppose, however, that the review was written by the editor of *Nature.*

In this, my second notice, promised in the last number but one, I propose to touch on some of the remaining subjects admirably dealt with by Professor Young, and to show that the sole subject worth referring to is not, as the reviewer in *Nature* seems to think, that investigation of the nature of the elements, in which Mr. Lockyer is understood to have been engaged (as that reviewer, indeed, asserts) during the last thirteen years,—with important results hereafter, we may suppose, to be made more fully known. (The communications to the Royal Society in 1878 and 1879 are understood to be merely preliminary.)

In the first place, there is an admirable account of the solar spots and faculæ, with absolutely the best description we have yet seen of the various stages of the growth and development of the larger spots. The discussion of the proper motion of the spots, and of the theories which have been advanced in explanation of the seemingly more rapid rotation of the equatorial regions is admirable. Here, though the subject does not require any difficult mathematical discussion, Professor Young's familiarity with mathematical methods of reasoning stands him in good stead. We may note, in passing, that Professor Young has not allowed his book to be disfigured by that ridiculous picture illustrating (save the mark!) the sun's axial inclination, which first appeared, we believe, in Guillemin's "Heavens," and has since done duty in so many astronomical works (by writers who should assuredly know better, and doubtless do). We could have wished he had substituted another, but it is certainly better to have no picture at all than to admit one conveying quite erroneous ideas.

Passing to the portions of the sun outside the photosphere we first note an important omission. In dealing with the lower atmosphere of the sun, and the beautiful observations by which it was first recognised in 1870, Professor Young calmly leaves out all reference to the name of the eminent astronomer to whom the discovery was due—Professor Young, then of Dartmouth, now of Princetown, N.J. We find an account of the invention of the method of observing the prominences without an eclipse, in which, while due credit, to say the least, is given to Janssen and Lockyer for their success in observing the bright lines of the prominences without an eclipse, the important share which Dr. Huggins had in the work is not overlooked, as it has been by some writers. "It seems to have been reserved," he says, "for Dr. Huggins to be the first to show practically that a still simpler device would" serve to show the whole contour and detail of a protuberance at once—that simple device being the one actually in use, and the only one which has ever been successful, the widening of the slit. The account of the various orders of prominences is very full of interest, and, though concisely written, it is the best and fullest extant.

Professor Young's discussion of the corona is, naturally, full of interest to myself. When I first made his acquaintance in America, the views which I had advocated respecting the corona were as yet but half accepted. In 1869, a certain degree of controversial energy had characterised the contradiction which Mr. Lockyer advanced against the belief which I then asserted to be mathematically demonstrable, that—apart from any further observations—the solar corona is a truly solar phenomenon, and not due to our own atmosphere. But during the eclipse of 1870, photographic observations showed that the theory which had been rashly characterised as "ridiculous," is, as a matter of fact, true. Even then, however, a section—a very small one truly—of solar students, maintained that only the inner and brighter part of the corona belongs to the sun, the outer part being a partly optical, partly atmospheric, phenomenon. The eclipse of 1871, during which two sets of six photographs, all agreeing together, were made, enforced a still further concession of coronal matter to the sun. For my own part, I was content to wait, the clear evidence of mathematics (elementary enough, too), assuring me that scarcely any appreciable portion of the light seen outside the body of the eclipsing moon could come from other than solar matter—that is, matter by the sun. It did not seem to me worth while to argue the matter; there had been enough, and more than enough, of argument, approaching sometimes to dispute; and I had had time to see that science can never gain by contention, though controversy has not always been unfruitful. At last the eclipse of 1878 disposed of all that had been in question. And in Professor Young's book, we find a picture of the corona of 1878, combined from various drawings, showing the real solar corona extending to a distance corresponding to some three million miles from the sun. Referring to these later views, Professor Young remarks that "as has been pointed out by Mr. Proctor, the observer at the middle of an eclipse is in the centre of an enormous shadow, generally from fifty to a hundred miles in diameter. If we grant that the air retains some sensible density and power of light reflection, even at an altitude of a hundred miles, and assume for the shadow a radius of only twenty miles, no particle of air illuminated by sunlight could, under these circumstances, be found within eleven degrees of the sun's apparent place in the sky. If there were no corona truly solar in its origin, there would, therefore, be around the moon a circle of intense darkness, twenty-three degrees at least in diameter; at the edge of this circle a faint illumination would begin, forming a luminous ring, something like a halo, outside of which the sky would be lighted by rays from an only partially hidden sky." I could have wished (it is not very important, but would have served to show the value of a little reasoning applied to observations, even to tolerably old ones,) that Professor Young had mentioned that this reasoning of mine was published in my "Treatise on the Sun," and insisted upon in papers communicated to the Astronomical Society several months

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before the eclipse of 1870, when observations, showing what I had already demonstrated, began to be made. My proof was not the less a proof that, if it was not *caviare* to the general, it was not so readily understood by them as the comparatively rough photographic demonstration; and the lesson needs insisting upon that very often we possess, already demonstrated, truths which a little careful reasoning will suffice to educe from observations already made. The cry for fresh observations is, in such cases, caused either by laziness or inaptitude, either by unwillingness to work out the truth from the observations in hand, or by incapacity to reason soundly and accurately.

I note that Professor Young accepts without reservation Dr. Henry Draper's important discovery of the existence of oxygen in the sun. I fully agree with him. His views respecting the elementary constitution of matter he has described in these pages. How far they agree (as the reviewer in *Nature* seems to suggest) with Mr. Lockyer's theory—or rather the theory advanced (earlier) by Professor Clarke, of Cincinnati, we leave our readers to judge.

Professor Young's account of the valuable researches made by his fellow-countryman, Professor Langley, into the question of the sun's heat and light, is full of interest. I note that Professor Young regards 10,000° Centigrade as an altogether more reasonable estimate of the sun's effective temperature than the monstrous numbers adopted by Secchi, Ericsson, and others.

I would fain dwell longer on this valuable work and on the fascinating subject of which it treats, but space will not permit. I recommend all who wish to know the present position of solar research to take this treatise for their guide—a guide thoroughly honest and trustworthy. The book is not a book to be tasted, or merely swallowed, but "to be chewed and digested." It has one fault (I think a somewhat serious one): Professor Young is exceedingly unwilling to claim his own work; as some of it has been rather unceremoniously claimed by others, he, in this, hardly does justice to those who in this country have urged his claims (of the justice of which, be it remarked, he is fully conscious, little though he cares to assert them). There is such a thing as carrying modesty too far. He also fails sometimes in expressing with sufficient confidence views which he is known to regard as unquestionably just. These, however, are, after all, very amiable faults. Perhaps I should not have noticed them if he had left me any others to note.

FOUND LINKS.

By Dr. Andrew Wilson, F.R.S.E.

PART III.

HAVING in my previous paper tried to show that the Mud-fishes were veritable links between the fish-class and the frog-class, we may now turn to the history of the latter group itself, by way of showing how, within its own limits, gaps and gulfs have been bridged in Nature's own way. The history of a frog is in itself an interesting study. It begins life as a tadpole, and lives, as most readers know, a perfectly fish-like existence. It is fish-like in form; its heart is two-chambered, and thus resembles that of the fish, and it breathes at first by outside gills. By-and-bye a broad fold grows over the gills, and ultimately covers them; whilst internal gills grow from the gill-arches. Meanwhile, the tadpole has been cropping the waterweeds by means of the horny jaws with which it is provided, and has been digesting its food within the long and spiral intestine which is the right and heritage of the vegetable feeder. Soon, however, the hind legs, which in the frogs and toads are the first to appear, are developed; and these are in turn succeeded by the front limbs. Lungs begin also to grow, as all lungs do, namely, in the form of two sacs or bags from the hinder or lower wall of the gullet. At this stage, the likeness of the frog to the fish has disappeared, and it closely resembles one of the common tailed "efts" or newts, which are familiar denizens of our ponds and pools. If it had retained its outside gills after its legs had become developed, the young frog would have exactly resembled that curious creature, the *Proteus*—found only in underground caves in Central Europe—or the curious Axolotl of North America. But the ways of frog-development do not permit it to remain in the guise and likeness of its eft-cousins. Whilst its lungs have been undergoing development, the heart has been approaching that of the frog-type, which possesses a three-chambered heart, as already observed. Then, as development is completed, the tail shrivels. Growing "small by degrees," it is represented in the adult frog by a mere rudiment; and, as the obliteration of the tail takes place, the young frog leaves the water and assumes the habits of a land-existence; breathing by lungs alone in its adult state, and exchanging, moreover, the vegetarian tastes of its infancy, for an insect dietary in after-life.

Now, the history of a frog is beset with questions of interest for the earnest mind that studies even its superficial features. Why, firstly, should a frog pass through these changes at all? is a very pertinent inquiry; and if this be capable of being answered, why, secondly, should its development run in the lines sketched out? If we start with the idea that animals and plants were simply "created" as we find them—and that view of matters is, of course, not yet displaced in unscientific circles—then, so far as I can see, no explanation whatever of the frog's development can be offered. "It is so, because it is so"—such is the logical dead wall that awaits the student who turns to the "special creation" theory for an explanation. There is no accounting for a supernatural creative fiat; we cannot give reasons for a "special creation;" in a word, we must, on this theory of nature, simply accept the fact of the frog's existence, and have done with it. But there exists the alternative idea of *evolution* and *descent*. What if it be admitted that one species or group of animals arises by natural variation and descent from another group? What if in the frog's development we are led to see a panorama—a moving picture, of the descent of its race? The reasonableness of evolution may thus, I think, become very apparent; contrariwise, I know of no other rational explanation of the frog's tadpole-stage, and its subsequent development.

What evolution, then, says is this: the frog is at first a fish-like, gill-breathing tadpole, with a fish-heart, because its earliest ancestor was a fish; and it is interesting to note that the young of some well-known fishes (*e.g.*, dog-fishes) breathe by outside gills. I have a beautiful specimen of two of these young fishes with their outside gills in my museum. Furthermore, the resemblances of the tadpole to the type of some primitive fish do not end with its outside aspect. Mr. F. M. Balfour says the anatomy of the tadpole points to its relations with the living lampreys, which, as every naturalist admits, must be fishes of a very ancient type. But, secondly, the tailed tadpole becomes four-legged, and it thus resembles, as we have seen, a newt or eft. The reason of newt-stage is evident if we assume that the frog-stage was attained through a newt-stage. Abbreviate the tail of the newt, elongate its hind legs, and with a few other modifications, we find

the higher frog to be represented. For the frog, let it be remembered, is the highest type of its class; and the evolutionist's contention is that it has ascended to that place and dignity by successively rising from fish to newt, and from newt to frog. The reasons for the "metamorphosis" of the frog are clear enough, on the principle that *development repeats descent*—not always clearly, it is true, and with much modification, but still plainly enough to reveal the ways of the "becoming" of the animal world.

If it is asked, Why do not all animals show their descent as clearly as does the frog? I reply, because their development has been modified. But it is none the less true that in the development of all animals we see glimpses of the lines of their genealogy. The great difference between a frog's development (or that of an insect or crustacean which also undergoes "metamorphosis") and that of, say, a fish which hatches directly from the egg, consists simply in the fact that the frog's development is mostly passed outside the egg, whilst the fish developes within the egg.

But it is interesting to note that the frog in itself thus serves to link together groups of its own class. Thus its own development—not to speak of that of the newts themselves—teaches us that the newts have arisen from the fish-stock, and that they represent a lower phase of amphibian life than do the frogs and toads with their shortened tails. Indeed, the study of the frog itself not merely proves to us its own evolution, but demonstrates an orderly sequence in the descent of its class—a sequence wherein the newt-type followed the fish, and wherein the frog-type, in turn, was evolved from the newt.

That some such explanation—or, at least, an explanation based on similar grounds—is the only feasible method of explaining the metamorphosis of a frog, may be stoutly maintained against all comers. Evolutionists may differ regarding the exact lines along which the descent proceeded. They do not differ regarding the main facts at issue, namely, that fishes are linked to frogs in more ways than one, and that the history of the frog-race, rightly viewed, is really a connecting-thread on which the various forms of living and extinct members of its class may be strung. In my next paper, I shall endeavour to trace the "links" which bind birds to reptiles.

INTELLIGENCE IN ANIMALS.

KEPLER'S (the mastiff's) claim to be looked upon as a reasoning dog may be regarded by some as being better based, perhaps, on what his master and mistress described as actual mathematical calculations. "Kepler," says the latter, "like his great namesake, is an excellent mathematician. Many distinguished men have been delighted with his performances in this direction. The mode of procedure is this: His master tells him to sit down, and shows him a piece of cake. He is then questioned, and barks his answers. Say he is asked what is the square root of 16 or of 9; he will bark three or four times, as the case may be. Or such a sum as '6+12-3 divided by 5,' he will always answer correctly: more prolonged calculations rather fatigue him. The piece of cake is, of course, the meed of such cleverness. It must not be supposed that in these performances any sign is consciously made by his questioner. None whatever. We explain the performance by supposing that he reads in his master's expression when he has barked rightly: certainly he never takes his eyes from his master's face." A singular performance, and one showing that some dogs possess not only keen vision, but keener powers of perception than most men. It would, however, be a mistake to regard Kepler's performance as illustrating the possession of actual reasoning power by animals. For certainly the calculations he seemed to conduct were conducted in reality by his master.

This intelligent animal showed excellent judgment when a large photograph of one of Landseer's dogs (that is, a photograph of a dog pictured by Landseer) was shown him. He showed his perception of the painter's skill by at once distinctly recognising that the photograph represented a strange dog, of whom, by-the-way, he manifested decided jealousy. Kepler knew the meaning of many words. He recognised clearly when his master was ill, and showed at such time real concern and sympathy. "He was exceedingly kind and unselfish to a little English terrier, called Tycho 'Brahe'"* (I quote again from Mrs. Huggins' interesting little sketch; only, as Kepler is dead, I change the tense in these few last sentences from the present to the past), "who often tried him, and to a very unamiable cat, who both formed part of the household in which he dwelt. Altogether, there was in Kepler's every look, and motion, and utterance, a noble and intelligent individuality which endeared him to all who knew him. Much might be learnt from him in many ways; and he was indeed worthy of a large share of an inclusive love—that love which loveth

'All things both great and small.'"

The question whether animals can count in any way, or discriminate, at any rate, between different numbers, is one about which different opinions have been expressed. We cannot consider that the question was answered (affirmatively) by Kepler's achievements, though he seemed to do more than count. On the other hand, the common opinion that a bird, whose nest has been robbed of all the eggs but one, is as well content with that one as with the entire set, is not supported by evidence, and, indeed, seems to have been devised to comfort the consciences of those who like to go birds'-nesting, but might be troubled with regret for the troubles of the parent birds, were it not for this ingenious theory. We all remember the remonstrance of Tom Brown, when East proposed to take all four of the eggs in the nest robbed by Martin, "No, no! leave one, and then she won't care," said Tom. "We boys," says the author, "had an idea that birds couldn't count, and were quite content as long as you left one egg. I hope it is so." However this may be with birds (and, on the whole, I incline to think even penguins, "boobies" though sailors call them, have some idea of the number of their eggs), the following story seems to show that dogs can count their young. "To my friend, Dr. Velasquez Level, a respectable physician of this city," writes M. A. Ernst, of Caracas; "and for several years a resident of the island of Margarita, I am indebted for the following touching instance of the sagacity of a bitch. Her owner, for some reason or other, had destroyed all the female puppies in two successive litters. On her having brought forth a third one, it was found that there were but three male puppies. The bitch, however, was observed to leave her whelps occasionally, and to return some time after. Being followed, she was discovered suckling three female puppies, which she had hidden under some brushwood, undoubtedly with the intention of saving them from the master's cruel hands." This, perhaps, is the most striking of all the cases we have yet considered. It would seem that when the female puppies of the first litter were destroyed, the mother either did not recognise the circumstance that all the male puppies were left, or else

* Tycho Brahe (the dog, not the astronomer) was commonly called by Dr. Huggins, Tyko Barky.

XUM

regarded it as merely accidental; for otherwise she would probably have tried with the second litter the plan she actually tried with the third. When the female puppies of the second litter were taken, she recognised the rule by which selection had been made. Thus she had up to this point reasoned well and with due caution, not adopting a conclusion until the evidence in its favour had become very strong and convincing. She had also shown a power of counting; for obvious though the result she obtained may seem to one of ourselves, capable of dealing readily with much larger numbers, yet if we conceive a mind so far inferior in matters of calculation to that of a savage (and such savages are known) who can scarce count up to five, and has to run through a process of calculation before he can say how many children he has, as the mind of such a savage is to that of a skilful mathematician, we see that to such a mind the process gone through by the animal in this case would be what a very profound calculation would be to the mathematician. In other words, we here have evidence that the difference between the mind of an animal and the mind of man is but one of degree, and that the animal is not more widely separated from man in this respect, than the lowest among men is from the highest.

NIGHTS WITH A THREE-INCH TELESCOPE.

By "A Fellow of the Royal Astronomical Society."

BY this time the student will have become tolerably familiar with his instrument. We propose to employ it to-night in the examination of some of the more striking objects in the glorious constellation of Orion (Map, pp. 204 and 205). And first we will turn it upon β Orionis

FIG. 9

RIGEL

or Rigel, fig. 9, which will furnish the young astronomer with good, if easy, preliminary practice in the detection of small stars in the neighbourhood of larger and more brilliant ones. Probably, at first, his eye will be dazzled with the brilliant blue coruscation surrounding Rigel itself; but a little careful attention will show just above and to the

FIG. 10.

λ ORIONIS

left of it a small bluish point, as shown in the figure. From Orion's foot he may proceed to his face, in which we shall find λ, a very pretty pair, tolerably close together, the larger star being yellowish, the smaller one more of a lilac hue. Our figure represents it as seen with a power of 120. The lowest, or most easterly of the three stars in the Giant's belt ζ, will next claim our attention, and to show this properly will be a pretty severe test of the excellence of the observer's instrument. As shown in our drawing, this

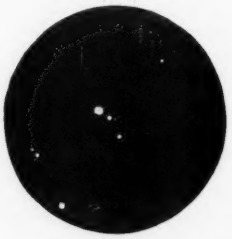
FIG. 11.

ζ ORIONIS

star is triple; the principal and second stars, with a power of 150°, being almost in contact, and the third below and to the right of them. Some considerable gazing will be required on the part of the beginner before he succeeds in making out the duplicity of the principal pair in this asterism. Our engraving may help him to understand exactly what to look for.

We now turn to σ, which will be seen beneath ζ in the Map. This is a triple, or, perhaps more correctly, a septuple star, all the components shown in our next figure being well within the same field with a power of 120.

FIG. 12.



σ ORIONIS

The object marked θ in the Map is one of the most wonderful in the whole heavens, consisting, as it does, of a mass of nebulous matter (now known to be intensely heated gas!)* surrounding, and seemingly physically connected with, a curious group of stars.

FIG. 13.

θ (AND 42 MINEBULA) ORIONIS

No woodcut can possibly do justice to this most marvellous object; but in our sketch, copied above, we have endeavoured to give some faint idea of its aspect as viewed

* Gas: but is it intensely hot?—Ed.

with a power of 80. The black gap leading up to the trapezium of four stars is known as "the fish's mouth." The nebulosity surrounding an isolated star, towards the bottom of the field, will be noted. The difference in colour of the stars forming the trapezium will be readily detected. There are a fifth and a sixth belonging to this group; but they are entirely beyond the power of such an instrument as that which we are using.

Having gazed our fill on this wonderful sight, and, furthermore, particularly scrutinised the trapezium of stars with the highest power at our disposal, we will lower the telescope a little to ι Orionis, a very pretty triple, in a fine field.

FIG. 14.

ι ORIONIS

Its aspect, as seen with a power of 120, is shown in fig. 14. The smallest of the three stars will require careful looking for before the unpractised observer will see it at all.

An even more difficult star is ρ^1 Orionis, represented in fig. 15. This will require a power of 150 at least, and, in

FIG. 15.

ρ ORIONIS

fact, as high an one as the observer possesses, to see the companion fairly. The small star is so faint and difficult with a three-inch aperture as to form a very fair light-test indeed. ρ^1 may be found by carrying an imaginary line through the three stars ζ, ε, and δ, in the belt, on which line, at double the length of the belt from δ, it will be found.

The last illustration we shall give is of 52 Orionis, a severe test of the separating power of such an instrument as we are considering. At moments of the finest vision, with the highest power at the observer's disposal, it will be seen as in fig. 16.

FIG. 16.

52 ORIONIS

Such are a few typical stars among a very mine of such objects in which the student may well search by sweeping for himself. Should he succeed in exhausting such a treasury in one night's work, he may turn his telescope down to Lepus, where, *inter alia*, he will find a pretty, and somewhat difficult pair in κ. This is the star to the right of λ, and just beneath ι, in the map on pp. 204 and 205.

BRAIN TROUBLES.

Partial Loss of Speech.

It will generally be noticed that some words are more troublesome than others when the mind is in this special state. Each person, probably, has his own peculiarities in this respect, and can (or might) recognise, from the misplacement or misspelling of particular words, the necessity for rest or change of occupation. It may be noticed, even in more remarkable cases of defective power of articulation, that some words suffer more than others. In the following case the patient had had an apoplectic fit, from which he recovered so far that his intellect and bodily strength were restored, but he could not speak intelligibly. He laboured under no paralytic affection, and could articulate freely, only, unfortunately, the syllables which he uttered with great apparent ease were meaningless. "When he came to Dublin, his extraordinary jargon led to his being treated as a foreigner in the hotel where he stopped, and when he went to the college to see a friend, he was unable to express his wish to the gate-porter, and succeeded only by pointing to the apartments which his friend had occupied." He perfectly understood every word addressed to him. He could read and understand written words and printed matter. "Having procured a copy of Andral's 'Pathology' in French, he read it with great diligence, having lately intended to embrace the medical profession." He expressed his ideas in writing with considerable fluency; and when he failed, it appeared to arise merely from confusion, and not from inability, the words being orthographically correct, but sometimes not in their proper places. We pass over other details indicating that he retained full possession of his mental faculties. The peculiar imperfection of language which he exhibited was thus tested by Dr. Osborn: He selected and laid before the patient the following sentence from the bye-laws of the College of Physicians:—

"*It shall be in the power of the College to examine or not examine any Licentiate previous to his admission to a Fellowship, as they shall think fit.*"

This being set him to read, he gave (at intervals of a few days), the two following versions:—

(i.) "*An the be what in the temother of the trothododoo to majorum or that emidrate ein einkrastrai mestreit to ketra totombreidei to ra fromtreido as that kekritest.*"

(ii.) "*Be mather be in the kondreit of the compestret to samtreis amtreit emtreido and temtreido mestreiterso to his eftreido tum bried rederiso of deiddaf drit des trest.*"

Here it will be noticed that the words "*be in the*" were correctly given on both occasions, except that on the first the word "*what*" is interposed between "*be*" and "*in*" (no doubt, merely as a question,) (What?). Again, the words "*of the*" were given correctly on both occasions. The word "to" was given correctly three times out of four, and on the fourth was probably nearly right, though lost in the written transcript in the word "*tum*" query *toom?*). Strangely enough, the words "*to his*," correctly given on the second occasion, were represented by the singular combination "*to ketra*" on the first. The word "*examine*," which occurs four times altogether, is represented by the following dissimilar combinations of syllables, (i) "*majorum*," (ii) "*emidrate*," (iii) "*samtreis*," and (iv) "*emtreido*," which only resemble each other in this, that all contain an "*m*" in the first syllable, and an "*r*" in the second. It is noteworthy again, and seems to prove the utter absence of method in this patient's defective articulation, that, whereas on the second occasion the three dissimilar words "*examine*," "*licentiate*," and "*admis-*

sion" were represented by the similar sounds, "*emtreido*," "*temtreido*," and "*eftreido*," they were represented on the first occasion by the utterly dissimilar sounds "*emidrate*," "*einkrastrai*," and "*totombreidei*." This patient knew when he spoke wrongly.

Reviews.

SCIENCE LADDERS.*

THESE little books are capital. They deal in the plainest possible words with their respective subjects, but what they describe they describe neatly and exactly. They are suited for the youngest readers, yet they contain a number of facts which many who are not very young will find full of interest. Even those who know most of the facts contained in these books will enjoy reading them simply because of the pleasant way in which the facts are presented. Take, for instance, the following account of the way in which hydras are born into the world. For a long time no one knew. "At last, however, a patient naturalist, who had long watched a hydra in a glass case, saw the growth of a tiny egg on its body, below the mouth and arms. Three days after the discovery of the egg, it was loosened from the body of the mother, and fell to the bottom of the water. When it first appeared it was cream-colour; it had now turned a bright orange. It remained at the bottom of the water for fifty-five days, and the only changes which took place in it during that time were that the outside skin became rough and the shape changed from round to oval. At the end of fifty-five days the egg cracked, and a baby hydra pushed out part of a soft, transparent, crystal-like body, quite round and smooth. Two hours afterwards this baby began to put out threads, and in seven days its shape was just like that of the mother hydra, only much smaller. This baby hydra took no food till it was more than a month old. It is now known that hydras do not always come from eggs, but grow out like branches from the bodies of grown-up creatures. From these branches spring yet younger branches, so that sometimes quite a family tree is made."

SCIENCE FOR ALL.†

THE object of this work so closely resembles that which we have in view in KNOWLEDGE, that it would be strange (considering who have been Dr. Brown's coadjutors) if the work itself did not meet with our approval. In the present volume, which is the fifth of the series, we find most interesting articles by Dr. Wilson, on Zoological subjects; by Prof. Duncan, on Earthquakes and Animals, old and new; by Dr. Mann, on Lighting, &c.; and by other writers on subjects with which they are respectively more or less conversant. The editor, Dr. Brown, discusses ably the question how plants were distributed over the earth, and in another article considers the question, What is a fruit?

Nearly all the articles are well written and well illustrated—many, quite admirably. The volume, as a whole, is a charming contribution to popular scientific literature, well-printed on excellent paper, and handsomely bound. Here and there are some shortcomings in style of treatment, and occasionally, but more seldom, in scientific accuracy. We may cite, as instances of the latter kind, Mr. Denning's statement that the Saturnian rings, if solid, may be maintained in equilibrium in the way suggested by Laplace. Nothing can be more certain than that the rings could not possibly be maintained in equilibrium as Laplace supposed. It is also not true that most careful measurements show the rings to be slightly eccentric. Some measurements have done so on particular occasions, that is all. It is clear, from Mr. Denning's account of the appearance of the rings, as supposed to be seen from the planet, that he has not given the matter even that cursory examination which Dr. Lardner gave it, otherwise he would assuredly have seen that in the vicinity of the poles no part at all of the rings could possibly be seen. Nor is it true that at night the rings would be seen as a vivid semicircle of light; during the winter half of each Saturnian year they would not be visible.

Apart from minor defects such as these, the volume before us is one which, whether regarded as part of the "Science for All" series, or judged by its intrinsic merits, can be warmly recommended. It is full of interesting matter, plainly worded, and, for the most part, exactly described.

THE SCIENCE OF THE STARS.*

WE admire Mr. Pearce's calm audacity. He sends to us for review a work on Astrology with as much confidence as if it were such a work as "The Courses of the Stars," by our esteemed correspondent Mr. Bazley, a work bearing a title which might cause it to be confounded with astrological treatises (instead of being one of the most valuable contributions made during the last few years to the science of the stars), while Mr. Pearce's book, instead of justifying its title "The Science of the Stars," advances gravely all the absurdest views of the astrological charlatan.

Perhaps the most remarkable feature of this work is the mixture of literary lore and utter ignorance of science. We have references to the sayings of a number of more than respectable writers, in company with statements based on the authority of such an ignoramus (to speak charitably) as the late Lieutenant Morrison, the Zadkiel of the notorious almanac bearing that name. One specimen of the manner of reasoning adopted by modern astrologers (the ancients had some reason for their errors) may be quoted—it will suffice, we should imagine:—

"It may appear arbitrary to take the moon as general significatrix (in mundane astrology) of the common people. Yet it would seem to have some show of reason when we remember that the Taybridge catastrophe, by which nearly one hundred lives were lost, only one or two of the passengers ranking above the class of 'common people,' took place on the very evening (Dec. 28, 1879) of the partial eclipse of the moon in the sign Cancer (which rules Scotland)—aye, and before the shadow had entirely passed away from the moon's disc. Ramsey avers that such an eclipse falling in Cancer denotes 'the death and slaughter of obscure, common, plebeian kind of people.' On July 12, 1870, a total eclipse of the moon, visible in Europe, took place. Three days afterwards Louis Napoleon declared war against Prussia. The slaughter in that war was horrible."

THE EFFECTS OF TOBACCO.

By Dr. Muir Howie.

PART I.

THE use of tobacco is becoming so extensive, that it is incumbent upon all who are interested in the health of the community, to devote some attention to the effect of such increased consumption. How does tobacco affect the human organism? Does it increase or diminish its capacity for physical or mental work? Does it tend to prolong life, or to bring on premature decay? Does it make life, as a whole, more pleasant and agreeable, or are its comforting and soothing effects rendered nugatory by subsequent irritability? Many such questions naturally arise in the mind of

* Science Ladders. Series I. No. 1.—Forms of Land and Water. Series II. No. 1.—Vegetable Life. Series III. No. 1.—Lowest Forms of Water Animals. By N. D'Anvers. Price 6d. each. (London: Sampson Low, Marston, & Co.)

† *Science for All.* Edited by Robert Brown, M.A. (Cassell, Petter, & Galpin, London, Paris, and New York.) Price 5s.

* "The Science of the Stars," by Alfred J. Pearce. (Simpkin, Marshall, & Co., London.)

the inquirer; but in the present paper I intend to narrow my consideration of the subject to one special point, viz., the food action of tobacco; and, in order to show you that I am not fighting against a man of straw, permit me to quote the words of one of our most eminent writers on the subject of narcotics—the late Dr. Anstie, who, in "Stimulants and Narcotics," says:—

"Next, perhaps, to coca in its power of replacing ordinary food, we must reckon tobacco. The power of this substance to compensate, to a certain extent, for the want of food, is very well known, but, strangely enough, it is generally assumed that this property of tobacco is dependent upon its power to disgust the appetite, by prostrating the nervous power of the stomach. A very little reflection should be sufficient to entirely discountenance such a view. There are very many substances capable of destroying appetite by a depressing influence upon the nervous system; such, for instance, as the salts of antimony, or the preparations of ipecacuanha; yet no one will pretend that the action of any such drugs would relieve the sense of faintness produced by fatigue, endured in the absence of food—an effect which tobacco undoubtedly produces in persons with whose system it agrees. The experienced sportsman, accustomed to tramp long hours over the heather in quest of game, would laugh at such an explanation of the effect of his favourite "cutty." He knows very well that it is by no mere disgusting of his appetite that he comforts himself for the indefinite removal of the prospects of dinner. By the time he had succeeded in depressing his stomach to the level of indifference to food, he may be sure he would have rendered himself incapable of continued strenuous exertion were tobacco effective only in this way. That tobacco is not an exact equivalent for roast beef, nobody knows better than the smoker; at the same time, it would be impossible to persuade anyone who had practical experience of the use of it to believe that its only effect is to depress nervous power. The fact is, that all such statements are made on the authority of persons either practically ignorant of the effects of smoking, or else naturally incapable, as some are, of deriving benefit from it. There are a few people whom no amount of care and skill exercised in the taking of tobacco, nor any moderation in the dose used, can save from unmistakable poisoning, whenever they indulge in it. These cases are rare, and they ought to be carefully separated from the evil results which are produced by mere unskilfulness in smoking, such as causes the trouble of beginners in the art."

Now in order to enter intelligently into the discussion of such an utterance as the above, we must pursue a line of argument like the following:—

1. What constitutes any substance a food? Does tobacco possess those attributes? If not,

2. Are there no substances of advantage in nutrition other than those properly termed foods? If not,

3. To what class of agents does tobacco belong, and what is its exact influence upon nutrition?

1. What constitutes any substance a food? We answer, whatever can be used either to build up the body or add energy to it is a true food. The human body, like the steam-engine, requires two classes of materials for its efficiency. It requires nitrogenous material, by which the machinery is built up, just as the steam-engine is made of iron, brass, &c.; and it requires carbonaceous and other material, whose combustion gives motive power, just as the steam-engine requires coal and water for purposes of motion. Every nerve and muscle in the body is a vast assemblage of cells, and each cell is filled with explosive material, ready to burst on the application of the slightest stimulus, and thereby to liberate its pent-up energy in order to conduct the vital functions for which it is adapted. The vital processes are thus conducted by a continued series of explosions, and so great is the heat generated by such explosions, that unless the human body were mostly composed of water, it would go off in smoke like a bomb-shell, or quickly disappear by spontaneous combustion. When one end of a nerve is irritated, a series of explosions runs along its entire length. If this nerve leads to the brain, it excites thought; if to a muscle, it excites movement by originating a series of explosions in the brain or in the muscle. Those of you who, as boys, have amused yourselves by setting fire to an end of a long train of gunpowder, and watched with delight the glowing force gliding, hissingly, along its course, will at once appreciate this explanation of nervous communication. When once the cell contents have liberated their energy by explosion, they are henceforth as useless as the washings of a gun or the spent ashes on the hearth, and must be swept out of the body as waste matter, to make room for a fresh supply of stored-up energy. It is through the medium of the ever-circulating blood that a continuous supply of such material is brought within reach of every cell in the organism; and it is by means of that same current that the waste matter is carried away which would otherwise as effectually extinguish life as an accumulation of ashes will extinguish the kitchen-fire. This waste matter we call poison, because of its power to interfere with vitality. I may mention, in passing, that it is among this waste matter that alcohol is found in the body of every man, be he the most ardent teetotaller or the most ardent spirit-drinker in these realms. Alcohol is the ashes which remain after the explosion of sugar in the body, and, like all other ashes, it is rapidly thrown out of the system.

We are sometimes told that alcohol must be a food, because it is found in the body. We might as well be told that spent ashes are good fuel because they are found in the fire.

Now, it is not maintained by any scientific authority that tobacco either assists in building up the tissues or in supplying them with explosive material; but such authorities do assert that it is useful in some other way. This brings us to our second consideration, viz.:—

2. Are there no substances of advantage in nutrition, other than those properly termed foods? We answer that there are. Just as the engine-driver cannot attain express speed without a liberal use of the poker, so the human machine cannot be kept in healthy activity without the administration of *stimuli*. Observe, I do not say *stimulants*, because that word has been corrupted, and now refers to a class of compounds which ought properly to be termed alcoholic narcotics. Alcohol is almost entirely used for its narcotic properties, and where thus used, cannot be admitted under the head of a stimulus. A stimulus is an agent which makes life more active, although it adds no energy whatever to the system; just as the poker will make the fire burn brighter, although it adds no heat or brightness of its own. Stimuli may be applied externally, or administered internally, and the more stimulus the body encounters among its surroundings, the less does it require mixed with its food. The man who takes a cold bath every morning before going to business does not require strong coffee to goad his nervous system to its daily toil. Those who have abundant open-air exercise may live entirely on vegetable diet, which contains but little stimulus; while those whose life is monotonous and sedentary require a more stimulating diet. But the healthiest stimulus is unquestionably the external. Open-air exercise, cold bathing, and pleasurable mental excitement will give sounder and better stimulation than the most savoury diet. Internal stimuli must only be resorted to when the external cannot be secured. There is one criterion by which you can always distinguish whether or not any agent is a stimulus, viz., by its power to increase the demand for food. The more you employ your poker, the more coal you burn; and just as you can extinguish your fire by a too vigorous application of the poker, and without adding fresh supplies of coal, so you may extinguish life by using too much stimulus without giving, at the same time, an increased supply of food. For example, if you feed a dog entirely on Liebig's extract of meat, which contains the stimulating properties of beef without much of the nutrient property, it will not live so long as if you fed it upon water alone. This proves that the extract of meat is a true stimulus, because it induces a greater necessity for food. It is thus useful for invalids with failing appetite, provided that true food be given at the same time. Now, tobacco is not a true food, neither is it a stimulus; for it rather diminishes the desire for food. Indeed, the boast of its advocates is, that it enables a man to do with less food, and even to do without food altogether for considerable periods.

3. How, then, does tobacco affect the animal tissues? It is not equivalent to the coal of the fire, nor to the poker. Where, then, can we find an analogous agent? Tobacco has the same effect upon the nerve-cells that water has upon a coal fire. Apply water in small quantity, and your fire will burn more slowly; apply a large-enough bucketful, and it will cease to exist. When the cook rakes up the ashes, and covers her fire before going to bed, she performs the same physical experiment as her master, who soothes his nerves with tobacco before retiring for the night. The cook wishes her fire to smoulder during the night. She therefore applies an agent which will check combustion by partially excluding oxygen from her fuel; her master applies to his nervous system an agent which diminishes oxidation, and thus seriously interferes with vital action. In both cases there will be less material burned, less coal and less explosive food. But is this a real advantage to the usefulness of the fire or of the human machine? The cook would be very late with breakfast if she trusted such a fire to boil the kettle, and the work accomplished by a brain much affected by tobacco is both small in quantity and inferior in quality. It is as difficult to send proper messages along a nerve which is under the influence of tobacco as it is to fire a train of damp gunpowder. "Praise God, and keep your powder dry," said the great Oliver Cromwell; "Praise God and keep your brain clear," would have been his burning advice had he lived in these latter days.

XUM

PROFESSOR GRANT ON METEOROLOGY.

At a dinner given at Glasgow by Mr. John Burns to the principal officials of several of the great railway companies of the United Kingdom, Dr. Grant, Professor of Astronomy in the University of Glasgow, gave an interesting summary of the present state of the science of meteorology. In the course of his remarks, he said:—A person wrote to Sir William Herschel requesting to be informed respecting the state of the weather during the next few months which would elapse after the date of his letter. The illustrious astronomer replied in terms to this effect: "The question of predicting the weather is one which is above the comprehension of astronomers and men of science in general." This letter was written about 100 years ago, and it expressed very concisely the immense difficulty of the problem. In those days the observations of the weather were very imperfect and limited in range. But a more hopeful view presents itself in the present day. During the last thirty or forty years, systematic observations of meteorological phenomena have been carefully made in all the countries of the civilised world. In many instances those observations have been discussed by men of science, and conclusions have been deduced from them which have thrown much interesting light on the climate of the countries to which they refer. Furthermore, the invention of the electric telegraph has supplied the means of rapidly confronting distant observations with each other, and of disseminating with equal rapidity the conclusions deduced from this inter-comparison. The result, then, is that in the present day the science of meteorology includes many valuable conclusions arrived at by careful induction from observation, and that even in the matter of predicting the weather, some progress has been made. It is, however, to be borne in mind that the attainment of this last-mentioned object is due rather to a sagacious interpretation of the observations, combined with the marvellous aid of the electric telegraph, than to a rigorous deduction from established scientific principles. In systematic observations of meteorological phenomena, the Royal Observatory, Greenwich, led the way in this country. Subsequently the observatories of Oxford, Liverpool, and Glasgow devoted attention to the same object. The establishment of meteorological societies in England and Scotland about the same time contributed also to the advancement of meteorology as a science. The Meteorological Office, originally a branch of the Board of Trade, commenced its labours in 1868, the council of scientific men under whose direction it is conducted being nominated by the Royal Society. Now, there are three leading objects which the Council have undeviatingly kept in view since the commencement of the existing organisation in January, 1868. These are:—1. Ocean meteorology. 2. Land meteorology of the British Isles. 3. Weather telegraphy. Allow me to make one or two remarks on each of those objects. I need not dwell upon the vast importance of ocean meteorology. It is upon the information furnished by this branch of the science that our ships, whether of the Royal or the Mercantile Marine, must rely for shaping their courses most advantageously over the trackless ocean. A fine field of scientific work is here opened up to ship captains who have a taste for the observation of meteorological phenomena. Any of such officers who desires to co-operate in taking observations is furnished with instruments for the purpose, and it is gratifying to learn from the annual reports of the Meteorological Council that the labours of many of them in this respect are much appreciated by the Council, as constituting valuable materials for subsequent discussion. Seven observatories have been established in connection with the Meteorological Office with a view to the advancement of the land meteorology of the British Isles. These are the observatories of Valentia and Armagh, in Ireland; Falmouth, Kew, and Stonyhurst, in England; and, finally, Glasgow and Aberdeen, in Scotland. The observations at each of these observatories are all obtained by means of self-recording instruments, and the tabulated results are regularly transmitted once a week to the Meteorological Office in London. The variations of the barometer and of the dry and wet bulb thermometers are recorded continuously upon paper by a photographic process which goes on night and day without intermission. The velocity of the wind is measured by its action upon a system of revolving hemispherical cups—an instrument invented by Dr. Robinson, the director of the Armagh Observatory. I was lately induced to make some calculations based upon the recorded anemometer observations at the seven observatories, with the view of ascertaining the mean hourly velocity of the wind at each observatory during the years 1874-5-6. The results of my calculations were these:—The mean hourly velocity of the wind for the three years in question was—for Armagh, 10·6 miles, 10·0 miles, and 9·8 miles; for Kew, 10·3 miles, 10·8 miles, and 10·8 miles; for Stonyhurst, 10·8 miles, 10·9 miles, and 10·7 miles; for Glasgow, 12·9 miles, 12·1 miles, and 12·4 miles; for Aberdeen, 13·3 miles, 13·5 miles, and 14·2 miles; for Falmouth, 16·8 miles, 17·0 miles, and 17·4 miles; finally, for Valentia, 18·2 miles, 17·7 miles, and 17·9 miles. It will be seen from this how nearly the annual mean results obtained at the same Observatory agree with each other. It will be seen, further, that, while Armagh, Kew, and Stonyhurst have the least wind, the lion's share of the wind falls to Valentia and Falmouth; while, again, the Scottish observatories hold an intermediate position in this respect. These results, it must be admitted, speak well for the observations on which they are based, and for the instruments with which the observations are made. They also afford us an interesting illustration of the presence of law as the regulator and controller of all the phenomena of nature. "Variable and fleeting as the wind" is an expression often used, and yet, when the winds at any place, which blow from all points of the compass, are gathered together, their aggregate velocity from year to year is found to be almost identical in amount. It cannot be doubted that important conclusions tending to throw light on the climate of the British Isles will result from a discussion of the observations received at the Meteorological Office from the outlying observatories. I have, finally, to make a brief reference to weather telegraphy. In considering this matter, the important fact must not be lost sight of, that the forecasts of the weather which emanate daily from the Meteorological Office are not given forth as rigorously deduced scientific conclusions by the eminent men of science who constitute the Meteorological Council. Had they been less fettered in the matter, they would probably have been more cautious, but they have wisely yielded to the public demand for such forecasts, and it must be admitted that in this instance the public instinct was in the right direction. The success of the forecasts, considering the difficulties which meteorologists in the British Isles engaged in such an inquiry have to contend with, has been very decided. During the last two or three years, as many as 75 per cent. of the storm warnings which have emanated from the Meteorological Office have been thoroughly successful. We may therefore confidently indulge the expectation that, with the progress of further researches, the percentage of successes will continue to increase. The Americans have shown great enterprise and skill in this matter. But it is to be borne in mind that they have an advantage in the inquiry which we cannot, from the nature of things, possess. For storms that come from the west—and these are the storms which really strike our shores—the Americans have a whole continent at their backs upon which to plant signals for the purpose of informing them respecting a coming storm; while we, on the other hand, have only the Atlantic, where no signals can be established. The present winter will hereafter be memorable for its storms. I may state that, in addition to Robinson's anemometer for measuring the velocity of the wind, an instrument which belongs to the Meteorological Office, we have also an anemometer by Osler, for the direct measurement of wind pressure, which is the property of the Observatory. During the storm of Friday, the 6th ult., this instrument recorded a pressure of 51 lb. on the square foot, and yet it bore the strain throughout admirably. I may remark in this connection, as an interesting fact, that during the great snowstorm which swept over London and its neighbourhood on Jan. 18, 1881, the Osler anemometer at the Royal Observatory, Greenwich, registered as high as 51 lb. on the square foot. The tremendous storms which occasionally sweep over a country are, no doubt, originally due to the agency of solar heat disturbing the equilibrium of the atmosphere; and, no doubt, the day will come when a close physical connection will be established between those grand phenomena and the origin of nearly all the energy on the earth's surface. But it may be a long time before this conclusion is arrived at. One of the most serious difficulties which meteorologists have to contend with consists in the imperfect knowledge which exists respecting the climatic conditions of the upper regions of the atmosphere. Mr. Glaisher, by his aeronautic ascents, did good service in this matter; but still much remains to be done, and it is probable that balloon ascents, notwithstanding the danger of such enterprises, of which we have had recently a sad illustration, will continue to constitute the only practicable means for arriving at materials which will serve to throw light upon this important question. In connection with this circumstance, I should not omit referring to the series of meteorological observations conducted during last autumn on the summit of Ben Nevis, under the auspices of the Scottish Meteorological Society. This spirited enterprise deserves to be renewed, and it appears to me to be one of those to which the Government might wisely give some support in conjunction with gentlemen of scientific proclivities throughout the country. I would finally remark on the desirability of establishing meteorological observatories on the east coasts of England and Scotland. It is noteworthy that from Dover to the Orkney Isles there does not exist upon the coast a single meteorological observatory, except the one established at Aberdeen in connection with the Meteorological Office. This is a state of things which ought not to

exist, and the want of such an institution was felt at the time of the Tay-bridge accident. Liverpool has set a noble example to other seaports in this respect, which is worthy of being imitated by the important seaports in the North of England, and by the ports of Leith and Dundee, in Scotland.—*Times.*

EASY LESSONS IN BLOWPIPE CHEMISTRY.

By Lieut.-Colonel W. A. Ross, late R.N.

Lesson II.

HAVING made his blowpipe, the student must now make, or provide himself with, a lamp, in the following manner:—The solid or closed end of an old iron gas-pipe, from 2 to 2½ inches in diameter, price about 2d., is the lamp, and a very excellent one it makes, as shown by my pupil M. Lombardi, of Argyll-place, Regent-street. The wick, a few strands of twisted cotton, held together and supported at one side of the gas-pipe by a piece of zinc-foil bent into the requisite shape, is now put in its place; and the fuel, consisting of any combustible hydrocarbon which solidifies on cooling, as pure beef or mutton fat, old "composite" or other candle-ends, cocoa-nut oil (from which glycerine has *not* been extracted), or all these together, added, and melted by heating the side of the lamp itself before the blowpipe by blowing the flame backwards on the iron rim; and here I must caution the aspiring blowpiper (or, as I prefer to call him, "pyrologist") against demolishing his work at this stage of proceedings by allowing the blowpipe-flame to play on the zinc-foil which he has just made! The wick, about the thickness of the end of a woman's little-finger, is best trimmed and cut with an ordinary pair of scissors, such as the American ones now sold for 6d. a pair.

So far, I feel sure, your working-men readers will admit that a blowpipe apparatus is by no means the expensive luxury it is thought to be; but the absolute necessity I am now about to mention, certainly does cost a little money, which will, I fear, somewhat strain his hard-earned and much-needed wages. I mean platinum wire, for old ends of which, however, half-price is allowed by Messrs. Johnson & Matther, Hatton Garden. This should be about the thickness of an ordinary horse-hair (I, myself, use it still thicker), cut into lengths of 3 in., and rolled into a ring about ¼ in. in diameter, at one end. This is best effected by the ordinary "cage-maker's pliers" of the ironmongers' shops; but anyhow, this ring must be carefully made, and as nearly a circle as possible, if only constructed on a pencil-point; unlike the slovenly figures recommended in some books on the blowpipe; some English works on chemistry even advising the use of a hook , which they tell you to fuse into a glass-tube by way of handle; to which piece of lunacy, I can only say, "Good gracious!" Here is a rough figure of a "platinum wire support," the natural size and thickness.

This wire is best held between the legs of a pair of ordinary "watchmaker's pliers," kept together, when required, by a little brass picture-wire rolled round the shank, so that this wire "strapping" may be slipped up and down; the "slipping up" closing the pliers tightly on the platinum wire, and enabling them to act as a handle for it; the "slipping down" opening the legs, and enabling them to act as pliers again. The pyrologist has thus not only a mere handle for his platinum wire, like the elegant ivory German ones sold at Freiburg, but an instrument by which he can effectually clean and straighten the latter for use. A mathematical pen also makes a capital holder for platinum wires. Now for my student's chemicals. Messrs. Herring & Co., the wholesale chemists, of Aldersgate-street, City (who, I dare say, have never heard of me or my blowpipes, but, for all that, I am a pleased customer of theirs), sell the exact kind of re-agent suitable for this part of my system of blowpipe analysis for 6d. an ounce, or less if taken in quantity; 2 ozs. lasted me four years. It is called "Glacial Phosphoric Acid," and cast in sticks. It is manufactured in Germany, and, I believe, contains from 15 to 20 per cent. of soda, without which it could not be cast; but it acts as a powerful acid before the blowpipe.

It should be broken into small fragments and kept in a wide-mouth stoppered bottle, as it is very deliquescent. To use it, shake out some of the smallest fragments on a white porcelain plate turned upside down, or any clean smooth surface. Make your wire-ring red hot before the blowpipe, and, touching the smallest pieces, which will thus adhere to the ring, heat them very gently, that is, about half an inch in front of the blue blowpipe-pyrocone, along with a few specks of oxide of manganese (of which two pennyworth will last most people a life-time). When the oxide is dissolved, which it is thus very rapidly, a charming amethyst-coloured "bead" is the result. Now plunge the bead into the middle of the blue pyrocone, and hold it there for half a minute; when withdrawn, it will be as colourless as a bit of pure glass. This effect may be repeated as often as desired.

EFFLUVIA AND HEALTH.

THE query of J. Maccinslane, No. 183, page 234, is probably answered by one of the quaint and wise remarks of the late Professor Brande, who, speaking of sulphuretted-hydrogen gas, said that "it is not the stinking gas, but the bad company it keeps that is so mischievous."

It is well known to all who have worked at elementary analysis in a laboratory, where this gas is much used as a general reagent, that no mischief arises from breathing an intensity of stink that would, probably, induce fatal disease if it came from a sewer. The same applies to dissecting-room effluvia. In my student-days, in Edinburgh, we used to say that the above-named gas gave us an appetite, but, if placed in a witness-box, I could not swear that this theory may not have been invented to justify suppers at "The Rainbow."

Had the bullock's blood used in Mr. Maccinslane's dye-house been left in a sewer, and sown with germs of fungi, bacteria, &c., the results would doubtless be very different, unless the other chemicals there used are sufficiently volatile to poison the poison.

W. Mattieu Williams.

THE USE OF FLEAS, &c.

THE query of "Amateur" reminds me of a paper I wrote many years ago, in which was revealed a discovery made during my wanderings in Greece. I slept—or rather reclined—in the capacious hovel of an Albanian farmer at Marathon, hard by the famous battle-field. My bed was the clay floor of the farm-house, my bed-fellows were my fellow tourist, our dragoman and horseboy, the farmer, his wife and five children, besides our three horses, the farm stock of poultry, and a population of the animals under consideration, exceeding in number the possibilities of a census.

During the night the children awoke at intervals, crying piteously, and the fond mother, knowing why, picked them up severally, laid them on her lap, and scratched them liberally all over. After this they slept for awhile in peace. Hereby was suggested my discovery of the use of fleas to mankind. These people were not addicted to washing, which is also the case with a large numerical majority of the human race. The less washing the more fleas, the more *et cæteras*, and the more scratching.

The unwashed majority of human beings require some substitute for washing, in order to effect the necessary removal of the shrivelled and effete epidermal cells. Scratching is such a substitute. But I have shown above that scratching varies directly with the supply of fleas and *et cæteras*, and inversely with washing. Therefore fleas and *et cæteras* benefit mankind as well as themselves, and the obligations between the species are strictly mutual. Q.E.D.

W. Mattieu Williams.

FISH "SOUNDS."

IN Dr. Andrew Wilson's very interesting paper on "Found Links" (Knowledge, Jan. 6, page 195), "the *swimming-bladder, air-bladder,* or *sound,*" are described as synonymous. There is a small mistake here, a misunderstanding of fishermen's technology, that may mislead some readers. The same mistake occurs in anatomical text-books. Dr. Wilson will readily understand the nature of the error by simply buying some "cod sounds" from any fishmonger. They are regular articles of separate commerce, salted by millions in Norway, and exported in small barrels.

He will find that the sound is not the air-bladder, but the *aorta,* or chief blood-vessel of the codfish, laid open, and with some of the larger branching vessels attached. This vessel, forming a stout membranous bag, is attached by its edges to each side of the under part of the spine of the fish, from which the Norse and Newfoundland fishermen tear or rip it when they split the fish for salting.

I have a theory of my own concerning the etymology of the word, viz., that it is of Scandinavian origin, like the commercial article, and is derived from *Söndre,* to sever or rip, from which we also derive our word *asunder.*

W. Mattieu Williams.

XUM

Letters to the Editor.

[The Editor does not hold himself responsible for the opinions of his correspondents. He cannot undertake to return manuscripts or to correspond with their writers. All communications should be as short as possible, consistently with full and clear statements of the writer's meaning.]

All Editorial communications should be addressed to the Editor of KNOWLEDGE; *all Business communications to the Publishers, at the Office, 74, Great Queen-street, W.C.*

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**** All letters to the Editor will be Numbered. For convenience of reference, correspondents, when referring to any letter, will oblige by mentioning its number and the page on which it appears.*

All Letters or Queries to the Editor which require attention in the current issue of KNOWLEDGE, *should reach the Publishing Office not later than the Saturday preceding the day of publication.*

"In knowledge, that man only is to be contemned and despised who is not in a state of transition. . . . . Nor is there anything more adverse to accuracy than fixity of opinion."—*Faraday.*

"There is no harm in making a mistake, but great harm in making none. Show me a man who makes no mistakes, and I will show you a man who has done nothing."—*Liebig.*

Our Correspondence Columns.

TELESCOPE.—VOLCANIC PROJECTILES.—THE EARTH'S INTERIOR.—WEATHER FORECASTS.—ANCIENT MAN (AND MODERN WORMS.)—"THE STARS IN THEIR COURSES."

[255]—"A Country Solicitor" (letter 237, p. 275) is quite right with reference to the superior handiness of a refractor, and if he is willing to spend, as he says, £60 in the purchase of an instrument which he desires to be able to move about with moderate facility, I should think that a first-class 3¾ in. telescope on a portable universal equatorial mounting, surmounting a strong and heavy tripod stand, would be the very thing for him. This would enable him, as he says, to go through Webb's "Celestial Objects" with profit and advantage. He would not, of course, be able to see *all* the objects described in the fourth edition, as some of them are definitely stated to be tests for instruments of large aperture; but he would find that a large proportion of the double stars and nebulæ included in Webb's lists would be well within his reach. I should be tempted to insist upon the equatorial mounting, as, apart from the ease and comfort of following a star by a single motion, many objects are unrecognisable without it. On the other hand, for less money, my querist might obtain a 6¼ in. reflector, equatorially mounted, too, which would give him superb views of the moon, planets, clusters, &c.; but this would be a massive affair, and there is always the nuisance of the mirror requiring re-silvering at longer or shorter intervals.

Mr. Rouse (letter 242, p. 276) may rest thoroughly assured that there is not an atom of foundation for the belief that a stone—or anything else—has ever been projected from a terrestrial volcano at the rate of 6·946 miles a second, the velocity needed to carry it into infinite space. The second part of this question is unanswerable, because the supposititious matter would never get out of the sphere of the earth's attraction until it got within that of some other body, and your correspondent does not say what that body is. If it be the moon, a projectile need only travel 2,141,956 miles above the earth! The query with which he concludes may be answered by saying that it has not been "proved" by astronomy that the earth is solid throughout, but that such solidity is rendered probable from the fact that she resists the tide-producing action of the moon exactly as a solid globe would do. Sir William Thomson has (according to Newcomb) shown that even were the earth less rigid than steel, it would, so to speak, exhibit tides itself, or be drawn out into an elliptical form; and then, earth and ocean moving together, we should have no tides at all. Moreover, the phenomenon of precession (now being described and explained in these pages by the editor) could scarcely occur were there nothing but a thin shell of rocky crust covering the molten interior of the globe, as such shell would slip round the fused nucleus, the liquid, in course of time, rotating in one direction, and the crust in another. Doubtless there are great cavities filled with molten matter, but these are insignificant compared with the size of the whole earth.

That "the Americans have not attempted to foretell fine harvest weather, or settled weather of any kind" (as stated by Mr. Donbavand at the conclusion of letter 247, p. 277), is the very reason why I place a certain amount of credence in their predictions. Barometric indications of a gale afford the most absolutely certain data which exist for predicting a meteorological phenomenon. Our own Meteorological Office gives us wild shots because they have to say something; the Americans many fairly trustworthy ones, because they have something to say.

Is Mr. Snell (reply 84, p. 279) thoroughly sure that the intercalated quartzose sand and loam of the Nile Valley is the habitat of worms?—because I am not.

I do not know whether the work is out of print, or whether (as is exceedingly probable), Mr. Bazley's own modesty has prevented him from referring in reply 184 (p. 279) to his own most beautiful book, "The Stars in their Courses." While heartily endorsing his recommendation of Proctor's "Library Atlas," which I regard as by very far the best one existing for the purpose for which it was designed, I may yet say that I equally look upon "The Stars in their Courses," as unparalleled as a means of learning the face of the sky. A Fellow of the Royal Astronomical Society.

WEATHER FORECASTS.

[256]—The remarks of Mr. Spiller (letter 124, p. 140) and "F.R.A.S." (letter 164, p. 207) ought not to pass without protest. The British predictions are not so bad nor the American ones so good as they represent, neither can the former be said to be mere "guess work," as insinuated by "F.R.A.S." The British forecasts, on the contrary, exhibit a large advance from a few years ago in our weather knowledge, and the general success which undoubtedly attends them is quite satisfactory to those who comprehend the grave difficulties of the subject. Prediction, in these islands, can never be otherwise than of a "general" character, and those who insist upon a particular forecast for every town, village, and villa in the United Kingdom, richly deserve the inevitable disappointment. The medical profession at times forecast the appearance of seasonable epidemics and the districts where they will appear, but what sane person would insist upon their walking over the said district and proving their prophetic instinct by pointing out the particular streets or houses where cases will occur. But meteorologists are asked to decide such minutiæ, and Messrs. Brown & Co. think it very hard that the thunder-storm occurring in their particular back gardens was not duly set forth in the day's bulletin. The fact is, that whilst the weather over defined districts maintains the same general features for a time, those features are subject to endless modification of a local character. More particularly is this the case with rain-storms, the non-prediction of which, by-the-bye, seems the only instance of failure given by Mr. Spiller. I have known rain-storms break out over areas of great extent, and within such areas there have been parts deluged and other parts with the finest weather (an instance occurring to my mind is the Whit Monday storm, May 25, 1874). I remember, too, during an excessively rainy autumn, a year or so ago, a "meteorologist" denying any unusual fall during the period, because his rain gauge had shown nothing extra. Again, I may mention an instance where the day's return from a certain district showed "very dry" weather, except at one station therein, where they had a whole month's rain in the twenty-four hours. Supposing another storm to occur like that on May 25, 1874, would Mr. Spiller or "F.R.A.S." expect the Department to say in what locality the rain would fall and in what it would not?

Singularly enough, your correspondents negative their own criticisms by the admiration they express for the American newspaper cablegrams. Putting aside the fact that they are quite as often "out" in their forecasts as our prophets are said to be (indeed, up to date—Jan. 18—four successive storms of theirs have failed to turn up), I must point out that the Americans take three days in point of time, and a coast-line extending from Spain to Norway within which their storms are to appear; and this, too, when the prediction is made after the storms have an actual existence. Will Mr. Spiller or "F.R.A.S." grant our own men equal indulgence? Three days' grace and almost unlimited areas for the fulfilment of their prophecies! Surely, after this, our own seers have little to be ashamed of!

I am not here disparaging the American warnings, for they fulfil to the letter my idea of what these forecasts must ever be—namely, general outlines both in respect to area and time. Beyond this, it is impossible at present to go, and, looking at the nature of the subject, I do not think we shall ever advance much further. In the meantime, it is to be hoped we shall not pour the whole of our felicitations upon foreigners, but accord some measure of praise to our own men, who have certainly not been less successful, and who, it is equally certain, have a much more difficult problem to deal with.

Albert P. Holden.

XUM

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STAR MAP FOR FEBRUARY

On January 29, at 10.30 p.m.
On February 2, at 10.15 p.m.
On February 6, at 10 p.m.
On February 10, at 9.45 p.m.
On February 14, at 9.30 p.m.
On February 17, at 9.15 p.m.
On February 21, at 9 p.m.
On February 25, at 8.45 p.m.
On March 1, at 8.30 p.m.
On March 5, at 8.15 p.m.

XUM

Our Star Map.—The circular boundary of the map represents the horizon. The map shows also the position of the equator and of that portion of the Zodiac now most favourably situated for observation, with the motions of the planets Jupiter and Mars upon them.—See the Zodiacal map on p. 225, No. 11.

XUM

CHINESE CALCULATION.

[257]—In reply to your question respecting Chinese Calculation, let me state that in Sir John Bowring's "Treatise on the Decimal System in Numbers, Coins, and Accounts," there is a good description of the Chinese swan-pan, or abacus, and mode of using it. He tells us that the following is the Chinese multiplication table, the simplicity of which recommends the whole scheme. Ten tens are a hundred; ten hundreds a thousand; ten thousand a wan = 10,000; ten wans a zih = 100,000; ten zihs a chaon = 1,000,000. He adds:—"At early morn, one of the first sounds heard in the shops of all the towns and cities of China is the shaking and cleaning of the swan-pans, preparatory to the business of the day. As, in Christian lands, the sound of the bells calls the worshippers to Church, as, in Mahomedan countries, the voice of the Muezzin from the minarets bids the devout Mussulmans to prayers, so, in "the middle kingdom," the rattling of the abacus announces that another morning's labours are begun.

With that instrument the Chinese youth has been as familiar as with his hermetrical classics, the first and most popular of his school books. From it he has received the most correct impressions of the relations of numbers to one another; and he has acquired the habit of moving the balls on the wires of his swan-pan with considerable dexterity and rapidity; wonderful are the ease and accuracy with which all calculations are made and recorded. In my own person I have had to settle a great variety of accounts with various classes of people in China, and I never remember to have detected an error; and in cases where my reckoning has disagreed with that of the Chinese, I have invariably found that their account was correct, and my own erroneous. In China it might almost be laid down as an axiom, that a mistake in an account is in itself strong evidence of fraudulent intentions. I have compared my observations with those of persons of the longest and most extensive experience as to the general correctness of Chinese accountancy; and my opinion has been fully confirmed, that among Chinamen intending to be honest, an error in reckoning is almost unknown.

HONG-KONG.

COMMUNICATION WITH THE MOON.

[258]—It was Gruithuisen, the continental astronomer, who maintained that in his observations on the moon, by means of a large achromatic telescope, he had perceived immense cities, edifices, and artificial structures, apparently the works of some being existing there; and from these appearances he concluded that the moon formed a home for reasonable creatures, with whom we might correspond. This idea he communicated to Gauss, who replied that correspondence might be carried on, but with signs which all rational beings must have in common; such as the right-angled triangle, with the properties of which (EUC. I., 47), the Selenites must be acquainted. The plains of Siberia, or the Great Desert, might be selected for the purpose; and the required figure—a right-angled triangle, a circle, or an ellipse—be formed by channels dug in the plain and filled with, say naphtha. Even if the object was not attained, the work would provide employ for the hordes which, objectless and homeless, roam those wilds. But, provided that the moon is inhabited, and that by rational beings, our intentions and efforts would be most likely misunderstood. But why correspond with those that are not?

HERBERT R. WELLER.

PROBABILITIES.

[259]—Seeing an article in a recent number of KNOWLEDGE on luck and probabilities, I venture to ask your opinion on the following problem, which has occasioned animated discussion in more than one instance:—

A bets B an even sum of money that in three cuts of a pack of cards he will turn up an ace, a knave, or a nine. Of course, it is immaterial what particular cards are decided on.

I have seen this done over and over again, the stake being a shilling, and it has rarely happened that either of the parties has lost or won to any extent; in fact, so close was the running, that an Irish bystander came to the conclusion that the odds were even.

I should very much like to see the correct calculation of the chances, as I have known many attempts made to solve the problem.

At first sight the chances appear all against the cutter, and the bet is usually taken with alacrity, but I imagine that the odds are slightly in his favour, though I don't know why.

Barracks, Dundalk, Jan. 14, 1882. CHARLES A. EDES.

[The solution would run somewhat as follows:—The chance that at a single cutting one of the three cards will be cut is $\frac{12}{52}$, since there are four of each of the cards named (no suit being mentioned in the conditions), or $\frac{3}{13}$; therefore, the chance that none of the cards will be cut at a given trial is $\frac{10}{13}$. Hence, the chance that not one of the named cards will be cut in three trials is $\frac{10}{13} \times \frac{10}{13} \times \frac{10}{13}$ (by the well-known rule for such cases),

$$= \frac{1000}{2197}$$

The odds are, therefore, 1197 to 1000 against A—that is, against turning up one of the named cards in three trials. The betting should have been about 6 to 5, or, more exactly, £5. 19s. 8½d. to £5 against A.—ED.]

MESMERISM.

[260]—As a constant reader of your valuable paper I should like to be allowed to ask a few questions about mesmerism.

My reason for so doing is because, on Saturday evening, I put one of my boys in what I believe is called a mesmeric trance. In joking with him, I told him to keep his eyes on a white spot of paper in the centre of a penny. I then made a few passes in imitation of those I had seen made by professional mesmerists, when I was astonished to see him drop into a trance, from which no amount of shaking, pinching, &c., could wake him. At first, being considerably startled, I did not know what to do, but recollecting how I had seen mesmeric subjects awakened, I blew on his forehead, tapped him on the head and said "Right." He at once awoke, but for some time (half-an-hour) was somewhat dazed, doing things almost unconsciously. Being rather startled at finding I possessed this power, I should like to ask a few queries, viz.:—

1. What is mesmerism?
2. What ultimate effect has it on the subject?
3. How is the subject restored to his senses?

And any other information on the subject that will be useful.

A STARTLED ONE.

Wigmore Schools, West Bromwich, Jan. 16, 1882.

P.S.—I may mention that I have previously been a disbeliever in mesmerism.

[As animals can be mesmerised—to use this rather absurd term for want of a better—it is certain that there is some physical effect to be interpreted. Unfortunately, many professional mesmerists mix tricks with what they can do without trickery.—ED.]

TERRACES IN DORSET VALLEYS.

[261]—Anyone travelling in Dorset must have observed on the sides of the valleys a number of terraces, sometimes rising on above another like steps, varying in number and also in size. I have noticed some as large as giant earthworks, with slope as smooth and top as level as any garden terrace. Others, again, are a few feet in length and about one in depth. They are a peculiar feature of that county, but I have seen occasional outliers in Yorkshire and Cornwall. What is their origin? I have seen it asserted in a newspaper article, in an off-hand way, that they are artificial; but there is no conceivable end commensurate with the immense labour the construction would have entailed. They might be old sea-beaches, if only they were all horizontal. Can anyone say whether there is any accepted theory about them amongst geologists?

S. H. W.

ICE.

[262]—In your current number, p. 252 (208), "A Fellow of the Royal Astronomical Society" appears to have made a slip. It may be a fact, though very improbable, "that ice does not vary in volume, as other solids do, with variation of temperature." But this is by no means a corollary from the other fact, that water in becoming ice undergoes a greater change of volume than most other substances do in the act of crystallisation.

There are other known substances—notably certain bismuth alloys—which expand on crystallisation, and remain permanently larger than before congelation. Does "A Fellow," &c., apply the same assertion to these, and does he consider that ice remains constant in volume at all temperatures at which it is ice, or does he think that it continuously expands with decrease of temperature?

AN ENGINEER.

SHORTENING OF THE DAY.

[263]—Laplace satisfied himself by reference to ancient astronomical records (meaning, no doubt, those of Hipparchus, who lived about 125 B.C.) that no alteration in the length of the day ha

XUM

taken place, even to the amount of $\frac{1}{100}$ of a second. Yet had there been a diminution of the earth's diameter, the day would have shortened. (See Whewell's "Hist. Ind. Sci.," 1837, vol. 3, p. 560, and vol. 2, p. 483.) Sir Charles Lyell says the diminution of the day is not $\frac{1}{100}$ of a second. ("Principles of Geology," 1867, vol. 1, p. 304.) "The earth's surface appears now to have reached a temperature which is virtually fixed, and on which the gain of heat from the sun is, on the whole, just compensated by the loss by radiation into surrounding space." (General Strachey, British Association, 1875, sections p. 181.) On the other hand, a scientific gentleman alleges that there are required "9,770,662 million cubic yards of crushed rock to produce the whole" of the fusing, heating, lifting, and wasted work at all the active volcanoes of the earth. Also he says:—"3. Heat wasted and dissipated in steam, &c., at volcanic vents, 5,842,848 millions of cubic yards." Who is correct? The present writer is no astronomer; he is

A Geologist.

[When Laplace expressed the opinion quoted, he supposed the lunar acceleration fully explained by his investigation. It has since been shown by Adams that about half still remains unaccounted for, unless the earth's rate of rotation is supposed to be diminishing. It was not till a very short time before 1867 that this result became generally known. Strachey's views and Mallet's, which are by no means opposed to each other, have very little to do with the question any way.—Ed.]

FLESH FOOD.

[264]—Allow me to reply to "Practical's" note of Dec. 30, 1881. Hear what Dr. W. A. Alcott says on this very subject:—"The only instance which, on a proper comparison, will probably be adduced to prove the incorrectness of these views, will be that of a few tribes of American Indians, who, though they have extremely robust bodies, are eaters of much flesh. But they live also in the open air, and have many other good habits, and are healthy in spite of the inferiority of their diet. But, perfect physically as they seem to be, and probably are, examine the vegetable-eaters among them of the same tribe, and they will be found still more so."

Again, "Practical" must compare men of the same nation and people, and not of different nations; compare Englishmen and Englishmen, and not English and Spanish, as climate varies.

T. R. Allinson, L.R.C.P.

[Let the mixed-food advocates and the vegetarians meet on this ground, and make out each a list of, say, one hundred of England's greatest and best belonging to each class. The former might start with Shakespeare, Milton, Spenser, &c., Newton, the Herschels, and so forth, five or six for each class of distinguished men. The vegetarians could then name an equivalent number of each class.—Ed.]

WATCH.

[265]—Can you or any of your correspondents explain the following circumstance, and suggest a remedy?

I have a very good watch, with chronometer balance, which on other people keeps admirable time, but will not go accurately when I wear it. Lately, I sent it to be cleaned, and had a similar watch lent me by the maker, which only varied thirty seconds a month. In a fortnight this watch had lost thirty-five minutes! Some people say this is caused by a magnetic condition of the body. Have others similar experience? and what can be done to prevent it?

Chronometer.

THE MINHOCAO.

[266]—The attention of the public is from time to time called to the supposed existence of a sea-serpent of enormous size, and the question of its existence has of late found a place in your columns. Probably few people have heard of the Minhocao, a worm of, according to some accounts, fifty yards length, and five yards breadth, covered with bones as with a coat of armour, and in its burrowings rooting up mighty trees, diverting courses of streams into fresh channels, throwing up heaps of earth, and in its course making trenches about three mètres in breadth. The reports of this animal, which has its existence in the highlands of the southern provinces of Brazil) seem well authenticated, and are as marvellous as those of the sea-serpent, if not more so. The accounts, however, as to the size and appearance of the animal are uncertain. It is supposed to be a relic of the race of gigantic armadilloes, which in past geological epochs are said to have been abundant in South Brazil.

The belief in this monster is not confined to Brazil, but is shared in by the people of Nicaragua, where a tradition of such a monster has existed from time immemorial; and as recently as the year 1866 a Nicaraguan *Gazette* gives a circumstantial account of an object much the same as the Minhocao. The accounts, however, of the Minhocao of Brazil are still more recent.

I have read that the Romans in their wars with the Carthaginians are said to have fallen in with a serpent 120 feet long, which dwelt upon the banks of a river and had tough scales.

As the existence of such an animal seems as interesting a subject of inquiry as that of the sea serpent, perhaps Knowledge may admit inquiries on the subject.

A. T. C.

FOSSILS IN METEORITES.

[267]—In No. XI. of Knowledge, you give an extract from the *Chicago Herald*, stating that fossils of sponges, corals, &c., have been found in meteorites, which, the extract goes on to say, are doubtless organic remains from an exploded planet. I have no doubt that they originally came from a planet, but the question is, What planet? The aforesaid *savans* say the exploded planet. But I believe it is infinitely more probable that this meteorite from which the fossils were obtained was originally expelled from the earth when first its solid crust was formed. The said fossils belong to the earliest geological epochs, and so far agree with this theory.

I believe Dr. Ball first set forth this theory, and I give nearly his words, extracted from the Editor's "Poetry of Astronomy":—"Meteorites are always angular fragments, even before they reach our air. Many meteorites have a crystalline structure, and, according to Haidinger, this indicates a very long period of formation at a nearly constant temperature—a condition only to be fulfilled in a large mass. . . . . Many meteorites show markings resembling those seen on terrestrial rocks, and caused by the rubbing together of adjacent masses." This, I think, sufficiently proves that they were expelled from some planet, and are not merely "dust of the system," which was, at some time or other, nearly captured by some planet in the process of aggregation.

And now we will attempt to find the most probable body for the origin of the aforesaid meteors.

To take the sun first. The requisite energy is certainly there, but where are the projectiles? The sun's body being certainly not solid, I think we may dismiss him as impossible. Then we find the moon. Here we find two objections. Firstly, if the projectile is given an orbit intersecting that of the earth, it will strike it in its first revolution, and so end its career for ever. If, secondly, its orbit does not intersect the earth, it will revolve round it for ever, and so never touch it. Thus, if we admit the moon to be the source of meteorites, its volcanoes must be still active, which is known not to be the case.

Thirdly, to take the case of the planets. Each planet may eject matter with such force as to pass out of reach of its attraction, but the chances are 50,000 to 1 against any such crossing the earth's orbit. But if we consider that every meteorite the earth expelled must cross the place of its ejection once in every revolution, we see that the chances are that the number of earth-born meteorites which reach the earth exceeds the number of those from other planets infinitely. Attraction of other orbs may, and no doubt does, cause the orbits of the earth and her meteorites to oscillate, but at some time or other they will come into collision. The fossils resemble those of the earth's earliest strata, and this was the surface of the earth in its intensely volcanic era.

To sum up, I consider the chances are that these meteorites were expelled from the earth itself, and carried some organic remains with them.

If there is a particle of evidence in the "exploded planet" theory, I should be glad to hear it. I do not wish to set myself up against these eminent *savans*, nor to contradict conclusions formed by years of toil and labour, but Knowledge is a medium through which the humblest may express their opinions, and as such an one I offer them.

Vignoles.

[It is hardly necessary for me to say that I share in large degree Mr. Vignoles' views, as the essay to which he refers will show. However, we have as yet no evidence of organic remains in meteors. The exploded planet theory, and the theory of two planets smashed in collision, seem to need no discussion. It seemed to me a jest when first advanced, and despite the gravity with which it has been recently urged by Sir W. Thomson, I can only regard it as a jest still. Despite the profound mathematical and physical learning of its author, even the assurance that he was in earnest would not lead me to regard it as deserving discussion. But like Professor Tait's notorious "sea-bird analogy," in explanation of the phenomena of comets' tails, it has not yet been advanced according to scientific rules. No attempt has yet been made to show that it explains observed facts, or that observed facts correspond with it in any definite way.—Ed.]

Queries.

[219]—The Atomic Theory.—Where can I find the best account of the atomic theory, and whether any theory has been put forward for the insolubility of certain substances in certain fluids?—Ernest L. R.

[220]—Chemical Analysis.—Could you inform me of the prices of the best books on qualitative, quantitative, volumetric chemical analysis, and food analysis?—Onward.

[221]—Natural Philosophy.—Which is the best "History of Philosophy" for students?—A. Summerson.

[222]—Telephone.—How could I construct a small telephone, and how do you join the wire together, so not to interfere with message sent?—G. H. Mortimer.

[223]—Aniline Dyes.—Could you kindly give me any information on the manufacture of aniline dyes, or tell me where I can learn anything about them?—Ernest L. R.

[224]—Electrical.—Would any kind reader of Knowledge inform me how to make the "rubber" for a cylindrical electrical machine; also whether it is necessary or not that the cylinder should be covered with shellac varnish?—A Greenock Student.

[225]—Ivy Leaves.—Of the various common plants which grow in these islands, ivy seems to me to show a wonderful variety of leaf-forms, depending upon the locality in which it grows. I should feel greatly obliged to any evolutionist who would explain the probable causes which produced these varieties.—E. C. R.—[The query is accompanied by drawings of several varieties, but as they are well known, it does not seem necessary to have the drawing engraved. We have sent it to Mr. Allen.—Ed.]

[226]—Gravity.—Reading the leading article on p. 241, I am quite at a loss to understand the following, taken from paragraph four:—"The most careful observation of the planets' motions reveals no evidence that gravity takes even any appreciable time at all in traversing the spaces separating the various members of the solar system from each other." As we can never have any atom of matter outside the influence of the gravitation of every other atom in the universe, how can the velocity of this influence be measured? I think it would be interesting to the readers of Knowledge if you or some one would favour us with an article explaining how this conclusion has been arrived at.—F. A. L. R. [It can be shown that if gravity occupied a measurable time in reaching a planet, there would be a force constantly hastening the planet's motion. But the matter requires more space than can be given in "Replies to Queries."—Ed.]

[227]—Barometric Oscillations.—Are wave-like oscillations of the barometric column in gales matter of usual observation? My attention has only been lately attracted to the fact. When the atmosphere is in a disturbed state during the passage of a cyclonic system, I set the vernier of the barometer accurately, and watching it closely for five or ten minutes, find that the column rises and falls say the thousandth of an inch, or, if falling, *falls by waves*, like the receding tide on the shore; the intervals between the waves being from thirty to sixty seconds or thereabouts. This may be a fact known to all observers, but, being new to me, I venture to ask the question, and to solicit a physical explanation of these oscillations.—G. R. W.—[If you consider that a barometer indicates atmospheric pressure, and that there must be waves of compression and rarefaction during a storm, you will infer that the oscillation you describe might have been expected. We do not know that it has ever been specially noted before.—Ed.]

Replies to Queries.

[151]—Jordan Barometer.—The inventor gives the sp. gr. of the glygerine as 1·26 at 60° F. (*vide* "The Glycerine Barometer," E. Stanford, Charing-cross, London, price 1s.). The glycerine employed is presumbly pure, as the above specific gravity corresponds to the natural density of the liquid, as stated in Watts's "Chemistry."—Edina.

[155]—Tortoise.—We kept a tortoise more than ten years. When he began to burrow for the winter (moving his body in a rotatory manner) we put him in a wooden box without lid, with a few square inches of carpet over him, and stowed him away under the sofa in the library. He was never deceived by premature fine days in January or February, but when the true spring was coming, he stood in the box on his hind legs, and with his fore-foot tapped to be let out, and we then turned him into the garden for the summer. He became very tame, and I often held him up in the open, and on speaking to him, "Come, Torty," he would put out his head to have his poll rubbed.—Charles Few.

[171]—Choanites.—Having examined many choanites polished in the mass, and cut into thin sections so as to admit of the employment of the microscope, and having compared them with the living sea-anemones, I had come to the same conclusion as Prester W., that the choanite is a silicified sea-anemone. The structure of the tubes is alike in both.—X.

[171]—Choanites.—The answer given to the above query, at the bottom of p. 251 of Knowledge, No. 12, is not satisfactory to me. Probably your correspondent and I do not mean the same thing when we speak of "Choanites." They differ widely from what we call Alcyonites. He may see specimens of the fossil Choanite in the British Museum, where they are described as "silicified sponges, from the chalk" and I think that those specimens show the spiral worm round the cup, but, of course, both the worm and the cup are in the fossil filled with coarse flint, and having been slit by the lapidary, the former appears as spots of flint, as intersected on each side by the wheel, and the latter as a straight, solid body of coarse flint, with which the lateral tubes are connected. If the Choanite, after all, is a sponge, as geologists say it is, it is easy to understand how the worm may be—I had almost said must be—a parasite.—Your obedient servant, Prester W.

[176]—The Blue Streaks in Brick Clay.—These are most probably disintegrated and decomposed sulphuret of iron, which is abundant in the plastic clay.—X.

[177]—Nautilus.—The use of the gut running through the chambers of the nautilus is to secure it to the first-formed chamber of the shell, as it has a periodical slipping of the muscle, which is repeated thirty or forty times during the growth of the animal, each time forming a transverse septum to resist the pressure from without.—C. W. Oldfield.

[185]—Cold Saturated Solutions.—Have you tried taking a *hot* saturated solution of this salt, allowing to cool with constant agitation, and then leaving it to stand over the night, at the temperature you want? Evaporate carefully down to dryness, without spirting, a measured quantity, say 50 grammes (not CO) of the solution, and weigh the residue; the result, multiplied by 2, will give per cent.—A Greenock Student.

[193]—Action of Thunderstorms.—Owing to the formation of ozone, which acts as a powerful oxidising agent. Ozone is formed by the passage of electricity through the atmosphere.—A Greenock Student.

[214]—Colours of Stamens.—I think T. Howse is scarcely fair in testing Mr. Grant Allen's opinion as to the original colour of flowers by plants grown in a conservatory, whose characteristics are so much modified by cultivation. When one speaks of flowers in a botanical sense, one means those grown in a natural state, *i.e.*, wild flowers. If T. Howse examines these, he will find not only that with few exceptions the stamens are yellow, but, in a large number of cases the corolla is yellow also, as buttercup, potentilla, dandelion, primrose, furze, &c.—F. D. H.

Fermentation in Beer.—I see "F.C.S.," in reply to "In Re" (p. 257), recommends him to read some work on brewing, preferably Dr. Graham's lectures, "if published." May I be allowed to state that they are published in the form of a pamphlet. I have not my copy at hand, unfortunately, or would quote publishers. May I also suggest that "In Re" read the articles, "Beer and Brewing," in Ure's "Dictionary of Arts, Manufactures, and Mines" (Longmans & Co.) and "Studies on Fermentation," by Pasteur (English translation published by Lyon, 175, Strand, London)? "On Fermentation," by P. Schützenberger (International Scientific Series, King & Co., London), is also well worth reading, but the thanks of English brewers are due more especially to Dr. Graham.—W. M.

The limiting depth to which light penetrates in water was some time ago stated to be 40 mètres for Lake Leman, by Prof. Forel, who used albumenised paper in his experiments. M. Asper has recently made similar experiments on the Lake of Zurich by a slightly different method. He used the photographic plates called *emulsion plates* (more sensitive than albumenised paper), and immersed them during the night of Aug. 3, to depths of 40, 50, 60, 70, 80, and 90 mètres. They were brought up after remaining twenty-four hours in the water, and treated with oxalate of iron. All the plates, without exception, were distinctly affected by the light. Thus the chemical rays penetrate in clear water to at least 90 mètres deep.—*Scientific American.*

XUM

Answers to Correspondents.

**** All communications for the Editor requiring early attention should reach the Office on or before the Saturday preceding the current issue of* Knowledge, *the increasing circulation of which compels us to go to press early in the week.*

Hints to Correspondents.—*1. No questions asking for scientific information can be answered through the post. 2. Letters sent to the Editor for correspondents cannot be forwarded; nor can the names or addresses of correspondents be given in answer to private inquiries. 3. No queries or replies savouring of the nature of advertisements can be inserted. 4. Letters, queries, and replies are inserted, unless contrary to Rule 3, free of charge. 5. Correspondents should write on one side only of the paper, and put drawings on a separate leaf. 6. Each letter, query, or reply should have a title, and in replying to letters or queries, reference should be made to the number of letter or query, the page on which it appears, and its title.*

C. M. Thompson. Very much gratified by your favourable opinion; our sub-editor (S. D. P.), in particular, thanks you. We note and watch the numerous leaks you mention.—J. P. Sandlands. About luck, in article; Huxley meant by extra-Christian, outside of, unconnected with, Christianity; you say "this is hardly the usual acceptation of extra"—pardon me, but I think it is (in extra-judicial, extra-ordinary, extra-logical, extra-mundane—in every word compounded with extra this is the sense). "Is it true," you ask, "that science is extra-Christian in this sense?" Surely; if not, it ought to be. What has science to do with Christian doctrines, or Christian doctrine with science? Next, as to Dr. Wilson's "Found Links," you ask how descent of air-breathing from water-breathing animals is proved by the linking of frogs and fishes through the mud-fish. For the life of you, you say, you cannot see how that, because one thing is something like another, it must have been its parent. Well, then, it was not for you Dr. Wilson wrote. Others showed that along a certain line of descent (according to the theory of evolution) links were missing; Dr. Wilson showed that this was natural under the circumstances, and he is now showing that along other lines the links still exist. You raise an entirely different question; for, if similarity of structure is no indication of relationship, there is no way of determining missing links at all, and it is idle to look for them. Geologists might turn up to-morrow the fossil of a creature presenting Simian and human features combined as we might expect to find them in a being midway between man and the supposed common ancestor of men and apes: but of what use the discovery, either way, if similarity is no proof of kinship?—Arthur Stradling. Our space does not suit an article on that scale; Ariel, Puck, gnomes, djinns, and pixies, are for regions where there is more dancing room.—G. A. M. Your query otherwise answered; we cannot undertake to forward letters from one correspondent to another.—C. Lloyd Engström. See, presently, article on "Fallacies."—Alex. Blake. Where there is no struggle for life, there *is* none. You show this clearly. Then you ask, Why should there be? Why indeed?—J. Horne. Your solution neat; see later reply, with general remarks on the problem.—C. A. E.—I can assure you I have not the slightest intention of allowing astronomy to drive out other subjects of interest. Like yourself, I was not pleased with the change which came over the journal you mention; it was one of the reasons why I ceased to contribute (as I frankly explained to the sub-editor). You need not fear that there will be any change in that respect in Knowledge, so long as it remains in my charge.—Phacopo. I cannot explain why your friend, forty years old and six feet in diameter (I beg pardon, that is the clock), can tell the time by a church clock at a distance of a mile. It seems clear he has excellent eyesight.—Edina. By stitial colures, I presume Mr. Bulley meant the solstitial colure.—Investigator. Having some sympathy for our contemporaries, we feel obliged, in common honesty, to say that the dialogue you quote ought not to be sent to any of them. Those who are likely to be misled by the arguments dealt with are simply those who have no reasoning power. Why reason with them then? I have seen a good deal of the gentry—the teachers are knaves, their followers otherwise.—Sirius. You are right; it was the sun and not the earth whose mass Professor Young gave as 2,000 trillions of tons.—Ocean. The lecture as given has not been republished—the theory dealt with is given in one of the essays of my treatise, "The Poetry of Astronomy." You say that the "top-gallant forecastle," about which I asked in my "Pleasant Ways in Science," is quite correct. But you define it just as I should define forecastle, and you tell me what the cross-trees are, saying they are topmast cross-trees, not top-gallant cross-trees. I have never heard either expression used, but always simply "cross-trees," and I have known what cross-trees were since I was ten years old. I still think the expression, "top-gallant forecastle," unusual, to say the least. I supposed the writer meant the cross-trees. It seemed to me as absurd as "cutting the water with her taffrail," in the "Red Rover."—W. A. C. I have not elected to vivisect Rover, but I have known of the better fruits of the practice, which, unless directed to such ends, I regard with as much abhorrence as you can. I admit that the argument about breathing is absurd; I did not urge it as otherwise; I only said it might as reasonably be urged as your own. I do not suppose my esteemed correspondent, F.R.A.S., meant one who had been limited to potatoes and turnip-tops; he used a familiar mode of speech. If I found anything in your letter which was more than simply a denial of his view, I would insert it. You cannot say I have not given due hearing to the other side.—Nelumbo. It is absolutely impossible, with our present circulation, to cut the pages. Which would you rather have, four pages more of original matter, or be saved the slight trouble of cutting the paper?—Student. It may be quite safely assumed that the total heat received from the stars is quite insignificant. Pray send account of the dog who inherited kleptomania.—M. Wyatt. The formulæ are practically identical, x being insignificant compared with r. Test the matter, if you doubt this. Thanks for the problem from Newton's "Universal Arithmetic," to which work, however, we have already referred readers.—A. W. D. Will try to find space for your suggestions about sleep and study. Only know of Bain's book against Phrenology.—R. B. Rowlinson. You are right, we should have said 2,700 B.C. not 2,700 years ago. The stars you name will be near the southern pole at different times. My Gnomonic Star Atlas is, I think, convenient for reference in such matters. At least, I always use it myself, and added the longitude lines and circles to make it useful in that way.—D. F. Barrett. Sorry it was not attended to, but the correspondence both with publishers and editor has been literally overwhelming.—Δικη. You are right; the question is whether vivisection is right or wrong, first, *at all*; and secondly, if under any conditions, then what those conditions are. Opponents are called sentimental; advocates are called brutal. Those who occupy a middle position are called names by both the extreme parties. Of what use is vituperation, anyway?—R. R. We do not know the work. "He combats the views of six of our leading scientists concerning evolution," does not sound promising. Good writers do not go about combating views.—Celt. Your theory that as the pyramid rose, the builders heaped up earth all around to enable them to put on the next layer, and afterwards cleared this matter away, seems only a little less ingenious than the theory that it was built from the top downwards. Have you formed an estimate of the tremendous extra labour the plan would have involved? Builders' measurements would be absolutely ineffective to preserve the accuracy of the orientation. An astronomer would not be content to discontinue the observations, but would make the orientation more and more accurate as the building rose higher.—Ellipse. You seem to overlook the fact that Mr. Thorp's first instrument is meant for ellipses too; it is only *set* for parabola.—H. Moulton. Inquire how many papers of large circulation cut their edges. The *Saturday Review*, the *Spectator*, the *Illustrated London News*, the *Graphic*, sixpenny papers, do not. *Punch*, *All the Year Round*, and a host of other papers less cheap than ours, might be cited as not cutting their edges. We never expected to continue the plan, any more than we expected to continue the issue of specimen copies by thousands. If your copy happened to be badly folded, the fault is unusual, so far as we can judge from the examination of a great number of copies taken at random. In the bound volume all irregularities will disappear; and we beg you to notice that the edges of the cut copies must again be cut when they are bound up. If you would but consider what we are trying to do in the way of cheapening science, you would be a little more generous than to ask for what is really only a luxury possible with comparatively dear papers, or with papers having only a limited circulation. We increase the quantity of original matter and the average size of the paper, and with reference rather to our promised than to our (very promising) actual circulation; you, and in all five correspondents, wish us to go back to the arrangements made when we were beginning. We beg to assure you, that except for the question of time, which absolutely prevents our acceding to your request, it would save us considerable expense to accede to it, if we at the same time diminished the original matter to the proportions which it had in Part I. You will see this clearly before the end of the next month.—James Moir. The phenomena are manifestly subjective.—Amateur Botanist. Surely botany has had a very fair share of our space. We could not publish "at extra cost" a supplement of star names and letters. Are not the letters and names given with the name of each constellation in the maps themselves?—J. N. Leigh. No evidence that earth's axis has changed.—R. C. Auld. You have, we trust, now received Part I. (that is, it reached you, we trust, before it was—as it now is—out of print). What happened was this: Your letter, enclosing stamps, was addressed to "editor"; it was sent to us, and its turn came a week or so after. It was then forwarded to the

publishers; and your later letter, being also addressed wrongly, is opened probably a week after the paper reached you.—H. ROLFE. Do not know Richardson's Conic sections. For analytical conics, Todhunter or Salmon; for geometrical, Drew or Tayler would suit, I think. You require a book suitable for fourth stage, at Kensington. Perhaps some correspondent will help.—ANONYMOUS, SILVERDALE. Darwin's works are published by Murray. Lyell's "Student's Geology" and "Principles of Geology" would be good works to begin with; say, first, the cheap edition of the former work published by Murray.—M. S. S. Already corrected; but thanks all the same.—A. BLUMENBACH, G. JOHNSON, H. STEINMAN, and others. Nos. 2 and 3 are now out of print. They cannot be reprinted; this has already been done three times, at a serious loss.—FRED. CRAMPTON. Tell us about the monkey, by all means.—E. MUIRHEAD LITTLE. We did not understand F.C.S. to mean that carbonic oxide is non-poisonous, or a supporter of combustion. Everyone with the most elementary knowledge of chemistry is aware that it is more poisonous than carbonic acid or the carbonic dioxide, and that it is no more a supporter of combustion than the latter. What we understood him to mean was that wood gas, after the carbonic acid in it had been converted into carbonic oxide, was changed in character, as described. Coal gas contains both CO and CO_2. Read as you understand it, "F. C. S.'s" communication would describe wood gas as simply carbonic oxide, which is, of course, simply absurd.—SATELLITE. The card of your compass must be badly suspended. Hold the compass horizontally, and if the card evidently "sticks," tap the sides till the card vibrates freely, it will then swing round till the north end points to the magnetic meridian (about 21° west of true north. A "Knowledge Almanac" may be thought of hereafter; at present, KNOWLEDGE occupies all our care.—A. J. MAAS. Thanks for snake stories.—T. J. Sun extinguishing fire dealt with in article on "Fallacies." The common idea that sun puts out fire is very different from the theory you deal with, that a fire burns less quickly in a room whose air has been warmed by the sun (after a certain considerable time). Sunlight admitted into a room does not warm the air in the room appreciably for some time; the fire is supposed to go out quite quickly, which certainly does not happen.—H. B. R. About tobacco immediately.—CL. A. SEGLER and F. GAUBERT. We knew Zares made the mistake you mention. Our answer was quite correct. There is no need of the isolation you suggest to produce conditions under which the least possible force will move the greatest possible mass. But at what rate? Zares said nothing about that. We know perfectly well where and how he is astray; but it is more useful to let him find his way to the right road, than simply put him on it and leave him *planté là*.—J. BROADHURST. American humour is as distinct from English humour as possible, as are American ideas about humour from ours; or, I should rather say, from ordinary English ideas on the subject. The actor approved by English tastes is often not liked in America; and *vice-versâ*, favourite American actors are often regarded with little favour here. Again, if you go with American friends to a play in England, you will find that what the English audience like they do not care for; while they at once select as the best actors those who are not regarded as absolutely in the first flight by most English critics. I must confess it seems to me they often show a more refined taste than ours,—perhaps because I find their views in agreement with my own. For instance, I have always regarded Rutland Barrington as one of the very best of our histrionic humorists, but his quiet humour seems very little appreciated compared with the more grotesque fun of other actors on the same boards. Now I find Americans quite at one with me in this view. The oftener they see him the better they like him. It seems to me, by way that so consummate a judge as M. Got, of the Comédie Française, was of the same opinion, even when he saw Barrington in a part so comparatively ungrateful as that of Captain Corcoran. Americans say that ninety-nine Englishmen out of a hundred fail to understand American humour at all. I remember an English fellow-passenger on board an American Pacific steamer, to whom the most outrageous absurdities were addressed with a certain grave calm (not solemnity), which should have been as suggestive of fun as Barrington's manner in telling us of Teazing Tom; but he took them in as confidently as he received the announcement of the day's run and the ship's latitude and longitude, and for aught I know, entered them in a big book about his travels which he was writing. He was "a perfect gold-mine of fun," a Californian said.—MUSICUS. You may be right. Conversation when music is going on may set the performer at his ease. He might be still more at his ease if the audience stopped their ears with cotton wool, and still more so if they all went out of the room. But the object in view when any one is invited to play, is not to set him or her at ease, but to hear sweet sounds discoursed. If a player is so inferior as not to be at ease, common sense suggests that he should not be invited o play. If, on the other hand, he plays well, you may depend he will not be set at ease, but very much the reverse, if he is not paid the compliment of silence. But to say the truth, no one who knows what music is either talks when others play, or does what you seem so to desire to do, talks when playing himself. I have heard many musicians speak of the habit some unmusical persons have of talking when music is in progress, and they have—one and all—denounced the practice as an offence to the audience and an insult to the performer.—P. F. A. Your assertion that a strong artificial light will put out a fire would entirely dispose of C. T. B.'s explanation, which yet you say is the only one you can arrive at. It is very easy to make a few experiments either on the effect of sunlight, or of strong artificial light. First get a good, steadily, blazing fire, with the shutters of a south-facing room closed, on a sunny day. Open the shutters for a quarter-of-an-hour, and note how during all that time the fire appears dull and languid. Close the shutters, and note that as soon as the eyes have become accustomed to the change, the fire appears as bright as ever. As you say, it does not appear so at once; but that is simply because the effect of sunlight on the eyes does not pass off at once. I do not myself agree with F.R.A.S. in regarding the idea that sunlight puts out a fire as a mere vulgar superstition, but rather as a very natural illusion. Moreover, there can be no doubt that solar heat, admitted long enough into a room to appreciably increase the temperature of the air, does, to some degree, diminish the activity with which a fire burns. This is no more a superstition than the perfectly correct idea that fires burn brightly in frosty weather. But you must remember that F.R.A.S.'s remark was altogether impersonal; it was not applied to the belief of the querist, for none was indicated: it expressed only F.R.A.S.'s view respecting the idea about which N. inquired.—J. F. The earth's axis is inclined 23° 27′ 16·60″ from a perpendicular to the plain of her orbit. The equality of action and reaction does not—exactly—*mean* "that if a man in a boat pushes against another boat ten times the weight of his own, the heavier boat would go one tenth of the distance of the lighter one"; though something like this would follow from the law: it means, more generally, that whatever pressure, strain, or action is exerted by one body on another, excites an exactly equal pressure, strain, or action in this other body, acting in the opposite direction.—M. J. HARDING. I have taken the liberty of forwarding your suggestion to the publishers of my "Easy Star Lessons." But surely it would be rather hard if an author were regarded as responsible for the pictures put on the binding of his book. As to the other work, considering who honoured the author in the way you mention, and the absurd remark he made, I should attach no weight at all to his opinion. How on earth could the writing of a scientific treatise be regarded as equivalent to a course of education specially fitting a man for ministerial duties? It was the hard hitting, I should imagine (and infer from the title of the book), which pleased the bishop. The works attacked seem to me perfect models of what scientific works should be, presenting accumulated knowledge, attacking none, courteous to all, even to opponents. A book attacking such works, and having for its title what implies that those attacked are godless and wicked men, carries its own condemnation on its title-page. That it should have run to the twelfth edition would show that there are many who enjoy such attacks, but would prove nothing as to the scientific value of the treatise.—R. LEE. We cannot find space for *all* the titles of Mr. Dallinger's essays. We fear the original query should not have been inserted. We might fill our whole number with replies, if many such questions were asked.—A. LUMMERSON. Have inserted one of your queries; as to the other, the person named is considered a dreamer in his own country, and has no scientific standing either there or here.—W. G. S. Certainly you heard the 800 puffs in nine seconds less time than was required to produce them. In the case of an approaching train, you always hear the sounds in quicker succession, and in a receding train, in slower succession, than if the train were at rest, or if you were a passenger by it.—S. H. W. Nay: if a cannon-ball were sent round the earth without any initial rotational motion, it would in each circuit present all parts of its surface towards the earth—not constantly the same face. The other passage seems carelessly written. If a mass of air came from the pole, in a moment, to our latitudes, nothing could bear the brunt of passing through it. But north-east winds come only from higher latitudes gradually to our own, and the deficiency of their original rotational motion is gradually made up by the frictional action of the surrounding air, earth's surface, &c.——T. S. V. P. Read Wallace's book on modern miracles if you want the sort of faith you refer to. We cannot admit the subject here until it has, or some part of it has, a scientific standing.—JAS. DEVLIN. Yes; we quite mean that the builders of the Great Pyramid could not possibly have oriented it so perfectly

XUM

as they did without telescopic aid, unless they had employed such methods as we find they actually did employ. The work was much more difficult than you seem to think.—Hugh. Every reflecting telescope (except those of the large Herschelian type) has two reflectors, which are not meant to be used separately. Telling me that the eye-glasses are both coloured red, one lighter, the other (as I might almost have inferred) darker, does not enable me to understand what is wrong. You should get an optician to look at the instrument.—Onward. Your first question out of our line.—W. A. Sadler. We think not; but if you will describe any experiments showing *how* sun rays might build up a planet, we shall examine them with exceeding interest.—Reporter. No room for shorthand discussions.—Ernest Groth. Much obliged. Have already, however, inserted a reply similar in effect to your own very complete one.—G. H. Mortimer. What can you mean? What *is* the substance of a light? In a gas-flame, the light is due to glowing carbon, the heat coming from the combustion of hydrogen.—W. E. B. F. C. S. Did not meet Mr. Severn in Australia; but he was well-remembered there, as were his lectures on astronomy. Do not know his address. Probably Mr. Ellery (Melbourne) may know.—E. B. V. I considered most of the phenomena you refer to in an article which appeared fifteen years ago or so in the *Intellectual Observer*. I may deal with it shortly in these pages, but so many subjects crowd in upon Knowledge, that it is hard to know which to take first. The apparent changes in brightness are only subjective, not objective, phenomena—they depend on the different brightness of different parts of the background. The shadow of IV. ought to be larger (including penumbra) than that of III., IV. being so much further from the planet.—J. A. L. R. How would your explanation apply to the moon? The enlargement certainly is an optical illusion, as measurement shows. The climate question will be raised in articles on Precession. Sun's proper motion wants an article for its proper elucidation. In my "Essays on Astronomy" it is discussed rather fully. The velocity has not yet been measured, and cannot be. Otto Struve's reasoning was quite unsound. As to the Vectors question, there is an obvious misprint. Is it not rather unfair to ask questions of that kind, leaving us either to occupy a portion of space for the query and replies, besides giving trouble to readers and to us in reading their answers, or else to send us to look up the book, which may or may not be ready to our hand? If all our readers who encountered such difficulties sent us queries, our whole space and more would be wanted for them.—T. W. Yes; the guillotine cutter works easily enough when fed; it is the feeding takes the time. The extra expense, when we are doing our best to give as much extra matter as we can provide for out of our scanty margin (or, rather, looking forward for the margin we hope to have when full grown) counts, of course, for nothing. The "nice little job" you have "cut out for our readers as well as ourselves too" we beg respectfully to decline. In an article such a matter may be discussed, not in replies to queries. —H. Snell. Thanks. Another letter pointing out the same mistake is in type.—Æolus and Boreas. It was your own joke about storms being sent us out of a gun. Did you think we "took you" seriously? Your questions too wide for the kind of answers you want.—J. A., L. M. B., A Carthusian, and others. Thanks for various solutions of the four fours problem, or approximations thereto.—Guinois. Yes, the errors are there, but one is an obvious misprint, and neither affects the result.—F. B. You are right; that "was what we intended to convey, officially."—W. B. Corrected already.—P. A. MacMahon, T. R., and others. The problem is not difficult with the Dif. and Int. Cal. "No analyst" wanted a solution he could follow. Thanks, however, for your solution.—J. Hammard. Much obliged for your solution of the messenger problem. We had already sent a geometrical solution to the printer's. At present we have scarcely room for mathematical essays; two have been waiting for room since the fourth number. We agree with you about leaving mathematical "problems" for a week, but not mathematical questions, simply because querists may be anxious for early reply. The simultaneous equations later.—E. J. Kennedy. Your solution will not do; the "by symmetry" assumption is unsound. Note, that you get, besides the solution, an equation of condition. Why should this equation hold?

The Future of Solar Physics.—The fundamental problems now pressing for solution are, "first, a satisfactory explanation of the peculiar law of rotation on the sun's surface; second, an explanation of the periodicity of the spots, and their distribution; third, a determination of the variations in the amount of the solar radiation at different times and different points upon its surface; and fourth, a satisfactory explanation of the relations of the gases and other matters above the photosphere to the sun itself—the problem of the corona and the prominences."—*The Nation.*

Notes on Art and Science.

The Earliest Date of a London Fog.—What the earliest record of a London fog may be I am unable to state; but since Mr. Hales has mentioned Evelyn's name in connection with a fog noticed by him in 1684, it will undoubtedly have struck more than one of your readers that twenty-three years previous to this, the delightful old diarist had published his "Fumifugium, or the Inconveniencie of the Aer and Smoak of London dissipated, together with some Remedies humbly proposed by J. E., Esq.," &c. "Published by His Majestie's command," M.DC.LXI. The warmth of expression used in this little tractate, now very scarce, would certainly point to the long-settled existence of the fog and smoke nuisance in the metropolis; indeed, the energy of the attack of this original Fellow of the Royal Society, his notice of the injury done to the health of the inhabitants, to the public buildings of London, and the furniture and "movables" generally, singularly coincide with the sentiments expressed about us in the present day. In the midst of our boasted civilisation and advance, the words of this accomplished gentleman of the seventeenth century are as much to the point on the question of the London fog and smoke nuisance as they were 221 years ago. Evelyn mentions ("Diary" Dec. 15, 1670) "the thickest and darkest fogg on the Thames that was ever known in the memory of man."—T. Carew Martin.—*Athenæum.*

Chloroforming during Sleep.—The possibility of chloroforming a person in sleep, without waking him, having been disputed in a recent murder trial, Dr. J. V. Quimby, of Jersey City, was led to test the question experimentally. The results were presented in a paper before the section of Medical Jurisprudence at the meeting of the Medical American Association, a few days ago. Dr. Quimby made arrangements with a gentleman to enter his room when he was asleep and apply chloroform to him. This he did with entire success, transferring the person from natural to artificial sleep without arousing him. He used about three drachms of Squibb's chloroform, and occupied about seven minutes in the operation. The second case was a boy of thirteen, who had refused to take ether for a minor operation. Dr. Quimby advised the mother to give the boy a light supper and put him to bed. She did so, and Dr. Quimby calling when the boy was asleep, administered the chloroform and performed the operation without awakening the boy. The third case was a boy of ten years suffering from an abscess, and the same course was pursued with equal success. Two important inferences may be drawn from these cases, Dr. Quimby said:—minor surgical operations may be done with perfect safety and much more pleasantly than in the ordinary way; and, secondly, a person somewhat skilled in the use of chloroform may enter a sleeping apartment and administer chloroform with evil intentions while a person is asleep. Hence the use of this drug in the hands of a criminal may become an effective instrument in the accomplishment of his nefarious designs.—*Medical Advance.*

Crustaceans and Light.—In a recent paper communicated to the Paris Academy M. de Merejkowsky describes experiments in which he sought an answer to the question, "Do the lower crustaceans distinguish colours?" His answer is that it is exclusively the quantity, not the quality, of light that affects them. Larvæ of Balanus, a cirrhipede crustacean, were employed, and some of the experiments were repeated on certain marine Copepoda. In a vessel that is quite dark these crustaceans are dispersed in all directions, but if daylight be admitted through a slit they collect about the entering beam. This occurs, too, when light of only one colour is admitted (a coloured liquid being put before the slit). These crustaceans do not seem to be blind for any colour (a result corresponding to what M. Bert observed in the case of Daphnides). But to find out whether they perceive colours as well, the following experiments were made:—Two slits being used for a beam of white and of coloured light respectively, the crustaceans preferred the former beam, all clustering round it if the coloured light was deep red or violet, and a large majority if that was of a brighter tint (yellow, green, or bright red). Comparing a beam of darker colour, as violet, with one more luminous, as yellow, the crustaceans always preferred the latter. With two slits admitting coloured rays of equal brightness, the crustaceans divided into two equal parts, whatever the colouration (the author thus compared bright red with yellow, green, and blue, yellow with green and blue, and green with blue); but directly the brightness was rendered unequal the groups of crustaceans became unequal, too. M. de Merejkowsky concludes, then, that in perception of light there is a great difference between the lower crustaceans and man, and even between them and ants; "while we see different colours and their different intensities, the lower crustaceans see only a single colour in its different variations of intensity. We perceive colours as colours; they only perceive them as light."—*Times.*

XUM

Our Mathematical Column.

MATHEMATICAL QUERIES.

[23]—A messenger M starts from A towards B (distance a) at a rate of v miles per hour, but before he arrives at B a shower of rain commences at A and at all places occupying a certain distance z towards, but not reaching beyond, B, and moves at the rate of u miles an hour towards A. If M be caught in this shower, he will be obliged to stop until it is over. He is also to receive for his errand a number of shillings inversely proportional to the time occupied in it, at the rate of n shillings for one hour. Supposing the distance z to be unknown, as also the time at which the shower commenced, but all events to be equally probable, show that the value of M's expectation, in shillings, is

$$\frac{nv}{a}\left\{\frac{1}{2}-\frac{u}{v}+\frac{u(u+v)}{v^2}\log.\frac{u+v}{u}\right\}$$

[Let the distance A be divided into p equal parts, each equal to δ, so that $p\delta = a$; then

$\frac{\delta}{v}$ = time in which M passes over space δ.

$\frac{\delta}{u}$ = time in which shower passes over same space.

Suppose the distance Z successively equal to $\delta, 2\delta, 3\delta \ldots p\delta$, and *in each case* suppose successively that the shower commences after a time $\frac{\delta}{v}, \frac{2\delta}{v}, \frac{3\delta}{v}, \ldots\ldots \frac{p\delta}{v}$ from M's starting, so that there are p^2 cases in all to be considered. Thus M's time is as follows in the following cases :—

. . . . . .

$z=\delta$; in p cases, $\frac{a}{v}$;

$z=2\delta$; in $(p-1)$ cases, $\frac{a}{v}$; in 1 case, $\frac{a}{v}+\frac{\delta}{u}$

$z=3\delta$; in $(p-2)$ cases, $\frac{a}{v}$; in 1 case, $\frac{a}{v}+\frac{\delta}{u}$; in 1 case, $\frac{a}{v}+\frac{2\delta}{u}$

. . . . . .

$z=p\delta$; in 1 case, $\frac{a}{v}$; in 1, $\frac{a}{v}+\frac{\delta}{u}$; in 1, $\frac{a}{v}+\frac{2\delta}{u}$; in 1, $\frac{a}{v}+\frac{3\delta}{u}$; &c.

in 1, $\frac{a}{v}+\frac{(p-2)\delta}{u}$; in 1, $=\frac{a}{v}+\frac{(p-1)\delta}{u}$

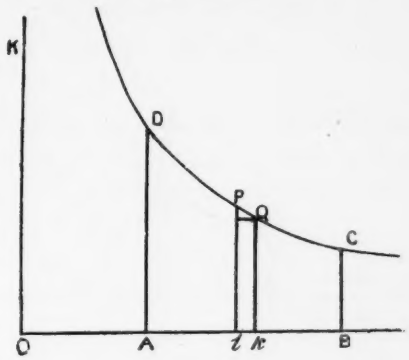
Thus, the sum of the amounts to be received, according to the conditions, is—

$$n\left\{\frac{p}{\frac{a}{v}}+\frac{p-1}{\frac{a}{v}}+\frac{1}{\frac{a}{v}+\frac{\delta}{u}}+\frac{p-2}{\frac{a}{v}}+\frac{1}{\frac{a}{v}+\frac{\delta}{u}}+\frac{1}{\frac{a}{v}+\frac{2\delta}{u}}+\cdots\right.$$
$$\left.+\frac{1}{\frac{a}{v}}+\frac{1}{\frac{a}{v}+\frac{\delta}{u}}+\frac{1}{\frac{a}{v}+\frac{2\delta}{u}}+\cdots+\frac{1}{\frac{a}{v}+\frac{(p-1)\delta}{u}}\right\}$$
$$=n\left\{\frac{p(p+1)v}{2a}+uv\left[\frac{(p-1)}{au+\delta v}+\frac{(p-2)}{au+2\delta v}+\frac{(p-3)}{au+3\delta v}+\cdots\right.\right.$$
$$\left.\left.+\cdots+\frac{(p-r)}{au+r\delta v}+\cdots+\frac{1}{au+(p-1)\delta v}\right]\right\} \text{ (A)}$$

Now
$$\frac{p-r}{au+r\delta v}=\frac{1}{\delta}\left[\frac{(p-r)}{pu+rv}\right]=\frac{1}{\delta v}\left[\frac{pv-rv}{pu+rv}\right]$$
$$=\frac{1}{\delta v}\left[\frac{pv+pu-(pu+rv)}{pu+rv}\right]=\frac{p}{\delta v}\left[\frac{u+v}{pu+rv}-1\right]$$
$$=\frac{p^2}{av}\left[\frac{u+v}{pu+rv}\right]-\frac{p^2}{av} \qquad \left(\text{since } \frac{1}{\delta}=\frac{p}{a}\right)$$

Thus series $(A)=n\left\{\frac{p(p+1)v}{2a}-\frac{p^2u}{va}\right.$
$$\left.+\frac{p^2u(u+v)}{a}\left[\frac{1}{pu+v}+\frac{1}{pu+2v}+\ldots+\frac{1}{pu+(p-1)v}\right]\right\}$$

Call the series withing the square brackets S, then the general term of $\frac{1}{pu+rv}$; or the general term of pS is $\frac{1}{u+\frac{r}{p}.v}$ and we have to find the sum of this series when r has all values from 1 to p, p being made infinite; or which comes to the same thing when $\frac{r}{p}$ varies through all values from 0 to unity. Now, supposing we know nothing of the differential calculus, we should, probably, at once see how this was to be done by using the well-known property of the rectangular hyperbola that the rectangle having asymptotes as sides, and a line joining centre and a point on the curve as diagonal, is of constant area. Thus, suppose we take OK, OB as asymptotes of a rectangular hyperbola DPC,



$OA=u$, $AB=v=AD$, and $Ok=u+\frac{r}{p}.v$, then we know that

$$\text{rect. } Ok.Qk=\text{rect } OA.AD=uv$$
$$\text{so that } Qk=\frac{uv}{u+\frac{r}{p}.v}$$

Hence, if we take $lk=\frac{v}{p}$ and complete rectangle Ql, we have

$$\text{rect. } Ql=\frac{v}{p}\cdot\frac{uv}{u+\frac{rv}{p}}=\frac{uv^2}{pu+rv}=uv^2 \text{ (general term of } S)$$

When p is made infinite, so that such a rectangle as Ql becomes indefinitely narrow, and the sum of all such rectangles between AD and BC is the area $ADPCB$, we have

$$uv^2(S)=\text{area } ADPCB=OA.AD\log.\frac{OB}{OA}=uv\log.\frac{u+v}{u}$$
$$\text{So that } S=\frac{1}{v}\log.\frac{u+v}{u}$$

Now the value of M's expectation is the total payable on all the possible events, divided by the number of events; or is series (A) divided by p^2 when p is made infinite.

$$=\frac{n}{p^2}\left\{\frac{p^2v+pv}{2a}-\frac{p^2u}{va}+\frac{p^2u(u+v)}{va}\log.\frac{u+v}{u}\right\} \text{ when } p \text{ is infinite.}$$
$$=\frac{nv}{a}\left\{\frac{1}{2}-\frac{u}{v}+\frac{u(u+v)}{v^2}\log.\frac{u+v}{u}\right\}$$

Of course, the solution thus given depends on the principles which underlie the differential and integral calculus. It does not seem worth while to master in each such problem the difficulties which result from avoiding the actual use of the calculus, except in this respect, that before the student begins to use the calculus he should so far accustom himself to deal with such problems as the above, that the real meaning, as well as the real value of the calculus, may be recognised. In dealing with the above problem we should simply get the general term of the series S, writing x for $\frac{rv}{p}$, and since $\frac{v}{p}$, when p is made infinite, is dx, we get

$$S=\frac{1}{v}\int_v^{\circ}\frac{dx}{u+x}=\frac{1}{v}\log.\frac{u+v}{u}$$

as by the geometrical method used above.—ED.]

[24]—1. Who introduced the symbol π? 2. What is the origin of the name "Courbe du diable," as applied to the locus $y^4-96a^2y^2+100a^2x^2-x^4=0$? 3. What is the origin of the name, "Witch of Agnesi"?* 4. Where can one find the best discussion of "Fourier's Series"? 5. How may an angle be trisected by means of the cissoid of Diocles?—W. W. BEMAN.

[25]—I borrow £100 from a Building Society, and repay principal and interest (compound) by 120 monthly payments of £1. 3s. 4d. What rate of interest am I paying?

* Agnesi says herself, vol. I., p. 381, "Equazione alla curva da descriversi, che dicesi la *Versiera*."

[Let r be rate per pound per month, so that at the end of a month £1 becomes £ $(1+r)$. Then, at end of first month £ $\frac{6}{5}$ is due; at end of second £$\frac{6}{5}$ + $(1+r)$ £$\frac{6}{5}$; at end of third, £$\frac{6}{5}$ + $(1+r)$ £$\frac{6}{5}$ + $(1+r)^2$ £$\frac{6}{5}$; and, finally, at end of twenty months—

$$[1+(1+r)+(1+r)^2+\ldots\ldots+(1+r)^{n-1}]\,£\frac{6}{5}$$

$$=\frac{(1+r)^{20}-1}{1+r-1}\,£\frac{6}{5}=[(1+r)^{20}-1]\,£\frac{6}{5r}$$

Now, £100 at rate r per £1 per month, compound interest, should be equal in value at end of twenty months to £$\frac{6}{5}$ paid monthly, as above, for twenty months. That is—

$$(1+r)^{20}£100=[(1+r)^{20}-1]£\frac{6}{5r}$$

$$(1+r)^{20}\,(6-500r)=6$$

Hence $20 \log.\,(1+r) + \log.\,(6-500r) = \log.\,6$.
We can find r tentatively from this equation. It is clear r cannot be greater than $6\div500$.—Ed.]

[26]—What will £10 annually at r per £1 per annum compound interest amount to to in twenty years?—J. R.—[We presume this is our correspondent's question. The method applied to previous question gives for this amount at the end of twenty years

$$[(1+r)^{20}-1]\,(£10\div r)$$—Ed.]

[27]—The value of a diamond varies as the square of its weight. A diamond falls and breaks into three pieces; what proportion does their probable value bear to that of the original diamond?—P. A. MacMahon.

[28]—Equation.—

$$\frac{a+x}{\sqrt{a}+\sqrt{(a+x)}}+\frac{a-x}{\sqrt{a}-\sqrt{a-x}}=\sqrt{a}$$

Mathematicus Novocastrorum.

[The Equations of "Rueverte" in our last seem to have been made "pie" of. We very carefully corrected the second (the first was rightly given), writing out in margin the left side of the equation afresh, but now the right side has entirely disappeared, and the first equation has gone wrong. It should be, I think,

$$\frac{x^2}{a}+\frac{y^2}{b}=\frac{a^2}{x}+\frac{b^2}{y}=a+b$$

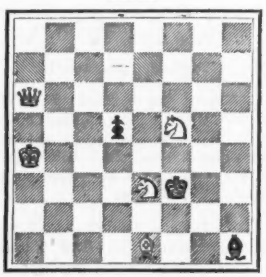
I cannot recollect what was on the right side of the other equation. Will "Rueverte" oblige by repeating it? Readers must not imagine that formulas which appear wrongly have been carelessly written or corrected. When I note that I had to correct several sheets of my book on Cycloids five or six times, and that even then errors appeared, they will see how difficult it is to secure correctness in the printing of mathematical matter.—Ed.]

Our Chess Column.

THE following problem gained the first prize in the late Problem Tournament of Design and Work. It is taken from the *Chess Players' Chronicle*.

By B. G. Laws.

Black.



White.

White to play and mate in three moves.

GAME AT ODDS.

First game in a match between Messrs. H. Erskine and W. Mead at the odds of Pawn and two moves.

White.	Black.
Mr. W. Mead.	Mr. H. Erskine.
Remove Black King's Bishop's Pawn.	
1. P. to K.4.	
2. P. to Q.4.	P. to Q.3.
3. B. to Q.3.	B. to K.3. [a].
4. Kt. to K.B.3.	B. to B. 2.
5. Kt. to Kt.5.	Kt. to K.R.3.
6. B. to K.3.	P. to K.4.
7. P. to Q.B.4. [b]	B. to K.2.
8. P. to K.R.4.	Kt. to Q.2. [c].
9. Kt. to Q.B.3.	B. to B.3.
10. P. to Q.5.	P. to Q.R.3. [d].
11. Kt. to K.6. [e].	B. takes Kt.
12. P. takes B.	Kt. to B.sq.
13. B. takes Kt.	P. takes B.
14. Q. to R.5. (ch.)	Kt. to Kt.3.
15. P. to K.Kt.3.	P. to B.3.
16. Castles Q.R.	Q. to K.2.
17. P. to B.4.	Castles Q.R. [f].
18. P. to B.5.	Kt. to B.sq.
19. Q. to K.2.	Kt. takes P. [g].
20. P. takes Kt.	Q. takes P.
21. K. to Kt.sq.	K.R. to Kt.sq.
22. Q. to K.3.	K. to Kt.sq.
23. Q. to Kt.6.	R. to Q.2.
24. P. to B.5.	B. to Q.sq.
25. Q. to Kt.4.	P. takes P.
26. B. to B.4. [h].	Q. to Kt.5. [i].
27. Q. takes B.P.	R. takes R. (ch.)
28. R. takes R.	R. to K.sq.
29. B. takes R.P.	P. takes B.
30. Q. takes B.P.	Q. to Kt.3.
31. R. to Q.6.	Q. to B.2.
32. R. to Q.7.	Q. to B.8. (ch.)
33. R. to Q.sq.	Q. to B.2.
34. Q. takes R.P. and White won.	

NOTES BY MEPHISTO.

[a] The intention of bringing the B. to B.2. is good enough, but we hardly think that Black can afford the necessary time for this manœuvre, we should prefer P. to K.3. followed by Q. to K.2., a line of play favoured by Mr. Potter. We, however, freely confess some ignorance as to this most cruel of all openings. A modern treatise on these and other odds would be welcome. In a game between Mr. Wayte and Mr. Hooke, at Leamington, the following occurred:—

P. to K.4.	P. to K.B.4.	Kt. to K.B.3.	P. to K.5.
P. to K.3.	P. to Q.4.	P. to Q.B.4.	

this line of play resulted unfavourably for the attack.

[b] White's attack relaxes a little, he ought to have *Castled* and then played P. to K.B.4.

[c] Black has emerged fairly from the first attack, and Castling at once would have been his best course. For, considering the odds given, he ought not to dread White's attack on his King's side, with the King in comparative safety.

[d] Sad loss of time.

[e] Well played. Should Black play Q. to B.sq., White would also obtain a strong position.

[f] Black misses a good chance of somewhat equalising matters. He ought to have played 17. P. takes P., followed by B takes Kt. and Castles Q.R.; he would then have either remained with Bishops of opposite colours or with Kt. against a B., and would have had a fair chance of drawing, *e.g.*, 17. P. takes P. 18. P. takes P. (P. to K.5. would not be good play.) B. takes Kt. if 19. P. takes B. Castles Q.R. or if 19. P. to K.B.5. Castles Q.R. 20. P. takes Kt. P. takes P. 21. Q. takes P. B. to K.4. with a good game.

[g] A desperate course, but there seemed no possibility of extricating the Knight from its helpless position.

[h] White conducts his game with good judgment.

[i] Black could not gain anything by playing his Queen away from his King's side.

"Synopsis of the Chess Openings." By William Cook. Third Edition. (London: W. W. Morgan, 23, Great Queen-street.)

The author of this treatise, who is himself a strong chess player, presents in this book the best forms of play according to latest practice, giving us the benefit of games played as late as 1880. We must give Mr. Cook great credit for his laborious collection of the best openings. What makes this book specially valuable is its practical tabular form. Every Chess player can at a glance see the line of play or variation he wishes to consult.

We extract the following as specimens of style:—

Cook's Synopsis of the Openings.

Table LXXVI.—Allgaier Gambit.

1.	P. to K.4.	2.	P. to K.B.4.	3.	Kt. to K.B.3.
	P. to K.4.		P. takes P.		P. to K.Kt.4.
4.	P. to K.R.4.	5.	Kt. to Kt.5.	6.	Kt. takes P.
	P. to Kt.5.		P. to K.R.3.		K. takes Kt.
7.	P. to Q.4.(1)				
	P. to Q.4.				
8.	B. takes P.				
	P. takes P.(2)				
9.	B. to B.4.(ch.)				
	K. to Kt.2.				
10.	Castles(3)			or	B. to K.5.(ch.)
	Kt. to K.B.3.				Kt. to K.B.3.
11.	Q. to Q.2.				Q. to Q.2.
	Kt. to B.3.				Kt. to B.3.
12.	Kt. to B.3.(4)	or	Q. takes P.(ch.)		Q. to B.4.(4)
	B. to Q.3.(5)		Q. takes Q.		B. to Q.3.
13.	Kt. to K.2(6)		Kt. takes Q.		Kt. to B.3.
	B. takes B.		B. to K.5.		Q. to K.2.
14.	R. takes B.		Kt. to B.4.		Kt. to Q.5.
	K. to K.4.		Kt. takes P.		B. takes B +
15.	Q. to B.3.		B. to K.2.		
	Kt. takes B.		R. to B.4. +		
16.	Q. takes Kt.				
	R. to B.sq.				
17.	Kt. to K.3.				
	Q. to Q.3. +				

(1) Mr. Thorold's attack.

(2) If 8. B. to Kt.2. 9. Kt. to B.3. 9. P. takes P. 10. B. to B.4.(ch.) 10. K. to Kt.3. 11. Kt. to Q.5.

(3) If 10. Kt. to B.3. 10. Kt. to K.B.3. 11. Q. to Q.2. 11. B. to Q.3. +.

(4) Mr. Freeborough's variation.

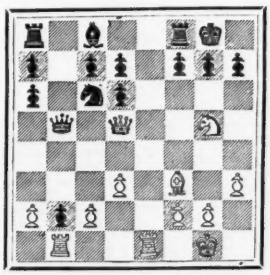
(5) Mr. Potter's defence.

(6) If 13. B. to K.3. 13. Kt. to Q.R.4 +.

Ending of an actual game played by Mephisto, illustrating the dangers to be shunned by weaker players.

AMATEUR.

BLACK.



WHITE.

MEPHISTO.

In this position White played.

R. takes P.	Q. takes R.
Q. takes B.P.(ch.)	R. takes Q.
R. to K.8.(ch.)	R. to B.sq.
B. to Q.5.(ch.)	K. to R.sq.
R. takes R. mate.	

Another specimen of the Chess skill of the late Mr. S. S. Boden

Select Reprint.—Bird's "Masterpieces," 66 (originally in Horwitz and Kling's "Chess Player"; and played soon after 1851.)—From the *Chess Players' Chronicle.*

[PHILIDOR'S DEFENCE.]

WHITE. Mr. Schulder.	BLACK. Mr. S. S. Boden.
1. P. to K.4.	1. P. to K.4.
2. Kt. to K.B.3.	2. P. to Q.3. (a)
3. P. to B.3.	3. P. to K.B.4.
4. B. to B.4. (b).	4. Kt. to K.B.3.
5. P. to Q.4.	5. P. takes K.P.
6. P. takes K.P.	6. P. takes Kt.
7. P. takes Kt.	7. Q. takes P.
8. P. takes P. (c)	8. Kt. to B.3.
9. P. to B.4.	9. B. to Q.2.
10. B. to. K.3.	10. Castles.
11. Kt. to Q.2.	11. R. to K.sq.
12. Q. to B.3.	12. B. to B.4. (d)
13. Castles Q.R.	13. P. to Q.4. (e)
14. B. takes P.	14. Q. takes P.(ch.)
15. P. takes Q.	15. B. to R.6. mate.

Notes.

(a) Mr. Boden was partial to this defence. It was a characteristic of his style that he did not mind a close position to begin with; he was not easily to be hemmed in.

(b) White's opening, though unscientific, is not without ingenuity. He intends of course, in answer to P. takes P., to sacrifice the Kt.

(c) He should have taken with Queen, and played as best he could for a draw.

(d) Not only preventing the advance of the Kt., but also doubtless shrewdly divining that his opponent intends to Castle on Q. side, and preparing a "concatenation accordingly."

(e) Winning a piece at least, if White has by this time discovered the danger to his King. This game has been selected as a specimen of Mr. Boden's felicity of combination in his lighter encounters.

*** The notes are by the Rev. W. Wayte.

A. J. MARTIN AND J. P.—You are right; 1. K. to K.3. is the key move to Probiem 5. We answered from recollection only of the problem, which was sent to us by Mephisto (with, of course, the correct solution). On turning to the position, we see that if Black checks with Bishop, R. covers, disclosing check; and after King has moved B mates, not R, as you suggest. In future, please address Chess Editor. We have not had a moment to open a chess board during the last three weeks, nor spare brain energy to go through games, or study positions without the board.—ED.

E. H. J.—All your solutions right.—ED.

DUNSTABLE.—Correct.—ED.

W. GODDEN.—No. 12 (wrongly called 11) is correctly given.—ED.

A. MACDONNELL.—All incorrect.—ED.

Our Whist Column.

BY "FIVE OF CLUBS."

IT may now be convenient to sum up the various leads, in such a form that they can be readily studied at a glance and easily remembered. We would invite those who have tried to retain in their recollection the multitudinous leads given in the books heretofore published, to note how simple the whist leads are when viewed as we have presented them. We venture to say—indeed, we *know*, having tested the matter—that a more perfect knowledge of the leads at whist can be gained in a week by considering when to lead Ace, King, Queen, and so forth, than in two months at least by the usual method of considering what card to lead from each of the numerous combinations which the cards may present. Moreover it is found in practice that a learner who has followed our method at once picks up the habit of interpreting the leads of others, whereas one who followed the other method is often a long time in passing from a knowledge of what he should lead to the ready recognition (instant recognition, it should be, after a little practice) of the meaning of any given lead.

SYNOPSIS OF THE LEADS IN PLAIN SUITS.

Lead Ace, from Ace, with four or more others, not including King; from Ace, Queen, Knave, with our without others; from Ace, two others (not including King), if you have reason to believe that your

partner has strength in the suit; and from Ace one other, whatever this other may be. The last two cases are, of course, forced leads.

After leading Ace, from Ace four or more, follow* with lowest, (unless you adopt Drayson's plan of playing lowest but one if there are more than four others). After leading Ace from Ace, Queen, Knave, follow with Queen if you have not more than one small one of the suit, otherwise follow with Knave. When you lead Ace from Ace two others (forced lead) follow with highest. Lead Ace from Ace, King, and others, when you have trumped another suit, lest your partner should trump your King, to establish a cross ruff.

Lead King, from Ace, King, and others; from King, Queen, and others (unless these others, being more than two, include the Knave); from King two others (forced lead), if you have reason to believe that your partner has strength in the suit; and from King one other (forced lead), whatever that other may be.

After leading King from Ace, King, and others, follow with Ace, unless you hold Knave, in which case you may sometimes—if the state of the score seems to render it advisable—change suit, that you may be led up to and finesse the Knave. After leading King from King, Queen, and others, if King makes, follow with small one, unless you hold Knave also, when follow with Queen (not with small one, because Ace may have been held up). When you lead King from King two others (forced lead) follow with highest.

Lead Queen from Queen, Knave, Ten, with or without others; from Queen, Knave, and one small one (forced lead); from Queen two others, not including Knave (forced lead), if you have reason to believe that your partner has strength in the suit; and from Queen one another (forced lead) whatever that other may be.

After leading Queen from Queen, Knave, Ten, follow with Knave, unless you have five or more, when follow with lowest of the Queen, Knave, Ten sequence. After forced lead from Queen two others, if Queen makes, follow with highest.

Lead Knave from King, Queen, Knave, and not less than two others† (not including ten); from Knave, ten, nine, with or without others; from Knave and two others (forced lead), and from Knave one other (forced lead).

After leading Knave from King, Queen, Knave, &c., follow with King if you have two small ones, with Queen if you have more. After leading Knave from Knave, ten, nine, lead ten if there is only one card below the nine, the nine if there are more. After leading Knave from Knave two others, whatever they may be, follow with highest.

Lead Ten from King, Queen, Knave, ten, with or without others; from King, Knave, ten, with or without others; from ten two others, or ten one other (forced leads). After leading ten from King, Queen, Knave, ten, follow with King if you have no small cards, otherwise with Knave. After ten from King, Knave ten, play a small one. After forced lead of ten, play your highest.

Lead nine from King, Knave, ten, nine; and in case of forced lead, from nine two others.

Lead a small card from all suits not considered in the above synopsis. Lead the lowest from four cards, the lowest but one from five or more (the lowest but two from six or more, if you care to adopt Drayson's rule); the highest from three or two small cards.

Note that it can scarcely ever happen that playing the lowest but one or two for the purpose of indicating length, can be mistaken by your partner for a forced lead from two or three small cards, or *vice versâ*.

We have already considered concisely, yet fully, the distinction between trump leads and leads from plain suits (see No. 12).

Observe that, short as the above synopsis seems, considering the multiplicity of Whist leads as usually presented, it would be very much shorter if it dealt only with original leads. For these one may say that all the beginner need learn is summed up in the following:—

Lead Ace from Ace and four others, following with small one; and from Ace, Queen, Knave, with or without others, following with Queen, if you have not more than one small one, otherwise with Knave. Lead King from Ace, King and others following with Ace; and from King, Queen and others, following with small one. Lead Queen from Queen, Knave, ten, following with Knave, unless you have five or more, when play lowest of head sequence. Lead Knave from King, Queen, Knave, and two or more, from Knave, ten, nine, with or without small ones. Lead ten from King, Queen, Knave, ten, and from King, Knave, ten, with or without others. Lead nine from King, Knave, ten, nine. In other cases lead a low one, the lowest if you have only four cards, the lowest but one if you have more.

Let the learner combine with this the general rule, that if he is obliged to lead from a weak suit, he always plays the best card of it, unless he has either Ace, King, or Queen, with two small ones. He now knows nearly all that he need know about leading from plain suits. All that he need at first notice about leading from trumps, is, that he can more safely play a waiting game in that suit, as his good cards in it cannot be lost by trumping; also that he must consider the trump card. The play in trumps is also apt to be modified by considerations depending on the state of the score, the position of the cards in other hands, and so forth.

* When we thus speak of second round, we do not wish the reader to forget that the first round may show it to be unadvisable to continue the suit; it may seem better to leave your own suit and lead your partner's, or to lead trumps, &c.

† In our last we inadvertently wrote "with or without others," instead of "not less than two others."

Whist Problem.—For the study of advanced Whist players we give the following from the "Westminster Papers":—

Z turns up Spade 7. A leads.

B's hand. *Spades*.—Ten, nine, six, five. Hearts.—Ace, Queen, four, two. Diamonds.—Queen, six. Clubs.—Ace, ten, eight.

First four rounds:—

A	*Y*	*B*	*Z*
C 6	C Kn	C Ace	C 3
H 9	H 5	H 2	H 10
D 8	D Kn	D Q	D 4
S Kn	S A	S 5	S 7

After these four tricks, B can place every card left in the players' hands, if they all play according to the rules usually followed. Show how he can do this.

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Contents of Knowledge *No.* 13.

	Page
Hyacinth Bulbs. By Grant Allen	261
Brain Troubles: Partial Loss of Speech	262
Dr. J. W. Draper. By the Editor. (*With a Portrait.*)	263
The Great Pyramid. By the Editor (*Illustrated*)	265
The True Story of the Moon	268
Babylonian Discoveries. By a Member of the Society of Biblical Archæology	268
Intelligence of the House Martin. By Henry J. Slack	269
Intelligence in Animals	269
Ghosts. By Andrew Wilson	269
Artificial Indigo	270
Science and Religion	271
Correspondence:—Chinese Calculation—Mind *v.* Doctors—Sea Serpent or Seaweed?—Marine Boilers—Intelligence in Animals—Electrical Images—Mortality from Cancer—Arranged Squares, &c.	272-278
Queries	278
Replies to Queries	279
Answers to Correspondents	280
Notes on Art and Science	281
Our Mathematical Column	282
Our Chess Column	282
Our Whist Column	284

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